BIOGRAPHICAL MEMOIRS OF FELLOWS OF THE ROYAL SOCIETY

VOLUME 6

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OF FELLOWS
OF THE ROYAL SOCIETY

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A photograph of the subject is printed with each notice

Agnes Arber

AGNES ARBER

1879-1960

Agnes Arber was born in 1879, the eldest child of a cultured family, whose members achieved distinction in different fields. Her father, Henry Robert Robertson, was an artist of Scottish extraction, whose father, a man of considerable botanical interests, also with gifts as an artist, had a private school at Slough. Her mother, Agnes Lucy Turner, was a descendant of Robert Chamberlain, who founded the china works of Chamberlain & Son at Worcester, and her mother's relatives included John Davidson, the African explorer, and George Fownes, both of whom were Fellows of the Royal Society. Her brother, Donald Straun Robertson, M.A., F.B.A., was Regius Professor of Greek in the University of Cambridge and author of various books on classical subjects. Her sister, Janet Robertson, was a portrait painter. At an early age her mother inspired her with an interest in plants and her father gave her regular drawing lessons from the age of three until she went to school. This early training developed her powers of observation, and laid the foundation of the skill and artistry which later formed such a notable feature of her books and papers.

At the age of eight she began lessons at the North London Collegiate School for Girls, where the renowned Miss Buss was headmistress. At that time the equipment and training in science at the school were exceptionally good, and Agnes became a very enthusiastic student of botany. At the age of 13 she read Massee's little book on *Plant life* in which she saw for the first time a picture of the cell and its nucleus. She learned about plant anatomy, and the gymnosperms and cryptogams from Miss Edith Aitken, a former student of Girton College, Cambridge, who taught botany at the school, and this teacher introduced her to the work of Goethe, to whose philosophy she devoted so much time and thought throughout her life. In 1894 her first paper was published in the school magazine. It was an illustrated study of the ivy-leaved toadflax, a little piece of research and original observation, whose recognition thrilled its author. Her botanical ability was at that time confirmed by the Cambridge Local Junior Examination, in which she obtained the first place in botany and was in consequence awarded a scholarship at her school.

While at school two events happened which greatly influenced her future career. She had the opportunity of studying a copy of *Lyte's Herbal* (1578) for a few days. This was an English translation of an earlier work by Dodoens and very finely illustrated. From this rapid schoolgirl study arose an interest which lasted through life and led to her publication in 1912 of a book on the evolution of printed herbals between 1470 and 1670, which has now become a standard work.

A second event was her meeting with Miss Ethel Sargant, who came to the school to read papers to the Science Club. This lady was a well-known botanist who had specialized for some years on the vascular anatomy of seedlings, working in a private laboratory at her house in Reigate. She invited Miss Robertson to work in her laboratory between school and college, and later during her long vacation from Cambridge. In this way an introduction to the technique and interpretation of serial sections was learnt.

Miss Robertson entered University College London, in 1897 and took her B.Sc. degree two years later. She then proceeded to Newnham College, Cambridge, with an entrance scholarship. At both Universities she obtained first-class honours in every examination taken, and at University College gained several prizes and medals. For Part I of the Natural Sciences Tripos at Cambridge she studied chemistry, physics, botany and geology, and worked at botany and geology for Part II. At University College she was taught by Professor F. W. Oliver and Mr (later Professor Sir Arthur) Tansley, while at Cambridge she went to the lectures of Professor Marshall Ward, A. C. Seward, F. F. Blackman, Francis Darwin and E. A. Newell Arber. She said that the most stimulating teaching at Cambridge was that of William Bateson, later Professor of Genetics, who was then in the full-flood of the rediscovery of Mendel's work. In the course of the teaching on gymnosperms by Professor Seward she studied the anatomy of Macrozamia as an exercise in research; the results of this study were published in *Proceedings of the Cambridge Philosophical Society* in 1902, it had not only served for her instruction but also was a great encouragement.

On leaving Newnham in 1902 she became research assistant to Miss Ethel Sargant in her private laboratory, and worked on seedling structures. From this time originated her interest in the seedlings of grasses, and a joint paper with Miss Sargant, on this subject was published. From 1903 to 1908 she held the Quain Studentship in Biology at University College and came once more under the influence of Professor F. W. Oliver, F.R.S. During this period the gymnosperms occupied much of her interest; the morphology and reproduction of *Torreya californica* were studied in 1904; the morphology of *Phyllocladus alpinus* was examined in 1906, and a phylogenetic study of the Taxoideae appeared in 1907. Oliver also encouraged her to study some fossil plants and this led to important papers on the structure of the seed of the palaeozoic gymnosperm *Mitrospermum compressum*, and an anatomical study of the palaeozoic cone genus *Lepidostrobus*.

On 5 August 1909, Miss Robertson married Edward Alexander Newell Arber (1870-1918), University Demonstrator in Palaeobotany at Cambridge. He was the elder son of Professor Edward Arber, D.Litt., F.S.A., famous for his *English Garner* and other studies in ancient English literature. Newell Arber was a member of Trinity College, Cambridge, with wide interests in plants. He was responsible for the collection of fossil plants in the Sedgwick

(Woodwardian) Museum, Cambridge, to which he added some 5000 specimens, and he lectured on both elementary and advanced palaeobotany. He maintained an active interest in the plants of the present day and wrote a book on *Plant life in Alpine Switzerland* (1910). His most widely known work was a joint paper with his friend John Parkin on *The origin of angiosperms*. He was an indefatigable worker on fossil plants, both as a collector in the field, and as a student of the collections made by others. His interests lay mainly in the upper palaeozoic floras, and in the stratigraphical occurrence of fossil plants. By his energy and enthusiasm he collected around him a number of research workers (including the writer) who made many contributions to our knowledge of the plants of the past. He also spent a considerable period arranging the fossils at the Natural History Department of the British Museum and in preparing a catalogue of the specimens of the Glossopteris flora.

The marriage was a happy one: they had one child, Muriel Arber, now F.G.S., who helped her mother in many ways. Husband and wife had many interests in common and Mrs Arber records how much she was helped by her husband, especially in the criticism and improvement of her papers and in the technique of research and publication.

The first and probably the most widely read of her books appeared in 1912. It was on *Herbals, their origin and evolution*, giving an account of the principal printed works published between 1470 and 1670, with specimens of the illustrations they contained, and short notices of their authors. She said that the idea of this book was suggested by Sir Albert Seward who was at the time Professor of Botany. There was an exceptional collection of herbals in the library of the Cambridge Botany School, which was available for her study and she produced a very fine book which will long be a standard work on the subject. Retaining an active interest in the subject over the years, she revised and largely rewrote the work for a second edition which appeared in 1938. This edition contained new material, such as a section on the herbals in Spain and Portugal, and a number of figures, in addition to the 134 of the first edition. Her study is an important contribution to the history of botany, and has done much to arouse an interest in the subject. At a later date she made contributions to the history of botany especially on Nehemiah Grew, in essays offered to George Sarton in 1946, and in a similar volume in honour of Charles Singer in 1953. Singer dedicated his *Short history of biology* to her, and to her also J. Hutchinson dedicated his Volume II (Monocotyledons) of the *Families of flowering plants*.

The quality of her work was soon recognized by the award of a Research Fellowship at Newnham College, Cambridge, which she held in 1912-13 and 1918-20. She was a Fellow of University College, London, and held the Keddey Fletcher-Warr Studentship, University of London, 1920-23. From 1936-38 she held a Leverhulme Research Fellowship. Her work was recognized in North America by her election as a Corresponding Member of the Botanical Society of America.

After her marriage and until 1927 she carried on research work at the Balfour Laboratory which at that period belonged to Newnham College. It had earlier been the Congregational Chapel and later was the first home of the University Biochemical Department. After 1927 she worked at her home in a room which was fitted up as a laboratory.

Under these conditions a continuous series of original observations was produced which formed the bases of over 50 papers, mainly published in the *Annals of Botany* and in *The Botanical Gazette*. These researches were chiefly concerned with the anatomy and morphology of monocotyledonous plants, which were studied from every point of view, and whose structure was depicted in many interesting figures. This work which occupied about 50 years of her life may be divided into four parts, in each of which a number of separate investigations were made which were later combined in a book, giving the main results in an abbreviated form, and with particular reference to the general principles derived from them. The first of these was a study of the structure and life history of aquatic plants published in 1920 with the title of *Water plants: a study of aquatic angiosperms*.

This described a variety of forms illustrating certain life histories and went on to consider the general anatomical and reproductive characters of the chief types. After reviewing the physiological and ecological condition of plant life in water, there was a series of chapters devoted to the study of water plants from the phylogenetic and evolutionary standpoints. The view put forward by Miss Sargant on the origin of the monocotyledons probably had considerable influence in leading Mrs Arber to investigate water plants. It was suggested that this group was a decadent race, of which some branches had been driven to an aquatic habitat to escape the severer competition on land. But the conclusion reached by further study was that no sound evidence could be found to support Miss Sargant's theory.

Much of the work which occupied Mrs Arber was concerned with monocot plants to which she was originally introduced by Miss Ethel Sargant. That skilled and eminent anatomist had accepted about 1910 an invitation from the Editors of the *Cambridge Botanical Handbooks* to write a book on *The Monocotyledons* for this series. Ill-health and advancing years made this impossible and shortly before her death in 1918 she suggested that Agnes Arber should take over the task. This led to the publication in 1925 of an admirable book devoted to a general survey of the group with numerous figures, one hundred and forty of which are original drawings by the author. The book opens with a valuable chapter on the principles of morphology and with an analysis of the philosophy of plant form; the two concluding chapters were designed to show how the monocotyledons appear to illustrate the principles discussed in chapter one.

This examination of the methods and object of morphological study was of great importance because, during the first quarter of this century, there was much confusion of thought about the philosophical principles underlying this type of work. Many English botanists believed that morphology was only useful in so far as it led to the discovery of phylogenetic lines. On the other hand continental botanists, especially those from France and Germany, following the traditions which had become established before the appearance of *The origin of species*, were interested in the study of form and structure for its own sake. If phylogeny was considered, it was based on the old views of plant structure handed on by tradition, rather than based on careful examination of objective facts bearing on the problem, and on the employment of logical reasoning. A completely new examination of the philosophy of morphology was needed, and this was commenced in the first chapter of *The Monocotyledons* and followed up in the remainder of her works.

Mrs Arber attempted to resolve the philosophical difficulty by drawing a distinction between the study of pure and applied morphology. The work on monocotyledons, like the later work on the Gramineae, was mainly concerned with the more general aspects of morphology especially as shown by anatomical studies. Most of the leading morphologists of the Continent had paid little attention to comparative anatomy, believing it to have little significance in morphological interpretation, but occasionally making use of it when it suited the writer's views. In England, however, much work had been done about the turn of the century on the comparative anatomy of pteridophytes and gymnosperms, both living and fossil, and this gave a quite different view of the significance of comparative anatomy. The preparation of this survey of the monocotyledons involved a considerable amount of original research much of which was published in some 20 preliminary papers, mainly concerned with the development and internal anatomy of monocotyledonous leaves and vegetative organs. All these were illustrated by many excellent figures.

After describing roots and axes, the foliage leaf received a further treatment. Although remarkably uniform in external features there is great diversity in its anatomical character, and in many cases genera and even species can be determined on structural grounds without any help from the reproductive organs, though this is not invariably true. The peculiarities of leaf form and structure seen in the group lead to attempts at its interpretation and Mrs Arber assembled the more important evidence bearing on the view that the original stock from which the monocots arose had leaves which were petiolar phyllodes. The comparative anatomy of the prophylls of the monocotyledons presents a number of difficult problems leading to the nature of the phylloclades of the Rusceae and the 'frond' of the Lemnaceae. These were elucidated by a careful study of the development and mature anatomy of the various forms, which cleared up many of the difficulties in the interpretation of these structures.

The study of seedlings has given rise to several hypotheses regarding the phylogeny of the monocotyledons and since Mrs Arber had been associated with Miss Sargant and seedling anatomy from her student days, the chapter on the monocot seedlings and their significance is of especial interest. She was highly critical of the view put forward by Miss Sargant and other authors, and discarded the theories based on the idea that cotyledons are organs *sui-generis* or anything but the first leaves of the plant. The anatomical and comparative study of monocotyledonous flowers and of the vegetative reproductive structures adds a further contribution of interest especially in the information which the author collected about the periodicity of flowering and the rhythmic alternation of vegetative and sexual phases of reproduction. The last two chapters, on taxonomy and its interpretation, survey some of the general conclusions which the author had reached during the course of this work.

During the course of this general study, which not only gave an account of a numerous and important group of plants, but was also an admirable introduction to plant morphology, Mrs Arber felt that it might be possible to get a clearer view of the questions arising out of this study if it were combined with more intensive work upon one of the great monocotyledonous orders. With this in view she returned to *the Gramineae* and she once said that during the next ten years these plants were seldom far from her thoughts. The plants available for study were very numerous, including the cereals, the bamboos and the many other grass forms, the problems presented are so inextricably interwoven with the history of man that the subject is much broader than a purely morphological study. It was preceded by a series of ten papers in *The Annals of Botany*, which, with their admirable illustrations, demonstrated many of the more complicated and less familiar topics. Seventy-nine of the blocks from this series of papers were used in the book together with many other original drawings. Many of the structures which are considered are explained by a series of drawings showing the same structure at different stages of growth or at different levels, rather than by verbal description. This forms a unique but very successful method of imparting information.

The works previously published on the grasses are extremely numerous and the bibliography with the references to works mentioned in the text occupied 43 pages. Beginning with an account of the cereals and fodder grass from the earliest times, the early chapters contain a most interesting collection of information both from the standpoint of world history from the earliest times and of economic botany. These chapters are written in such a clear and easy style and are so delightful to read that they formed a unique beginning to a book packed with technical information yet always interesting. When the structure of the vegetative and reproductive parts were described they were made clear and intelligible by the numerous line drawings in the author's inimitable style. While dealing with almost all of the main types of structure, both vegetative and reproductive, facts and views were interspersed among the purely structural information, giving it a great deal of general interest, with information about the ecology of the species, and their economic importance in various parts of the world.

Nearly twenty years of continuous investigation and thought about a group of plants with considerable variation in form, but at the same time with many characters in common, led Mrs Arber to a philosophical view of plant morphology rather than to a speculative phylogeny. This aspect of thought can be seen to be gradually developing in her work. She concluded her great work on the grasses in the following way. 'This slight sketch of the trends revealed in the grasses, opens up the idea that this group is a microcosm in which are displayed in little, all those tendencies of the corresponding macrocosm—the Monocotyledons—which can be developed within the definite, though elastic, framework of the ordinary type. . . . What is the *meaning* of the differences that separate the Gramineae so delicately, yet so definitely, from any other order, and that so prevail that a grass remains a grass however freely the type may vary? To attribute these differences to genic constitution is an "explanation" of a merely descriptive kind; it enables us, indeed, to assign a place to them in the mental framework which we impose upon reality, but in so doing we have shelved, not solved, the problem.'

About 1930 Mrs Arber began a series of studies on flower structure in which she investigated the vascular structure of a considerable number of forms and used the results as guides to the morphology of other parts. In 1937 she summarized the morphological views which had been expressed about floral structure, and their relation to the anatomical structure, an important critical survey. Several years before this she had examined the views put forward by Miss E. R. Saunders on the morphology of the angiosperm carpel, based on vascular anatomy: Mrs Arber found that this revolutionary theory of the carpel was not justified by the vascular structure actually present.

By 1942 Mrs Arber's publications were more than 70 in number and covered a very wide field, they gave valuable accounts of anatomical structure and critical discussions of the interpretation of the facts recorded. Her position in the study of botany was recognized by her election as a Corresponding Member of the Botanical Society of America and in 1946 she was elected to the Fellowship of the Royal Society: only two other women had previously received this honour. She had been a Fellow of the Linnean Society of London since 1908, and had served on the council of that Society from 1915 to 1919. She was awarded the Linnean Gold Medal in 1948.

During the second world war Mrs Arber found it impossible to maintain the small laboratory in her house and her active mind turned more and more to philosophy and to the quiet contemplation of the facts which she had observed during the work of the previous twenty-five years. *The natural philosophy of plant form* (1950) was the outcome of this study and is perhaps her most important book. When one examines a plant structure or compares it with that of another species we cannot make use of mathematical logic as in the physical sciences. What mental processes are employed in forming a unified conception and what is the history of these modes of thought?

The average student is ignorant of the mental processes employed and of the history of these modes of thought. It is easy to imagine a phylogenetic tree as suggested by the theory of evolution, in which the relationships of organisms are objective and the validity of an actual historic tree is assumed. But morphology long antedated the Darwinian theory and in fact Darwin placed considerable confidence in the theory put forward by Goethe on quite different grounds. Even today many of the ideas of morphology of the flowering plants are based on the doctrines of Aristotle as gradually developed during the centuries, and most of the continental work on plant morphology has been the direct outcome of Goethe's thoughts. At the present there is a hopeless state of confusion between the views whose basis is a series of mental concepts, with no objective basis, and those which are based on objective facts.

Mrs Arber, using the type concept, reviewed the structure of the flowering plants and put forward a series of suggestions based on an examination of the facts of comparative structure. Her book is a very interesting survey of the essentials of structure of the flowering plants, collected from published papers and from original observation. It is concise, well illustrated, and it supplies at last an account of the form of the flowering plants which is philosophically sound, being based on typology, but agreeing with the conclusions reached by research on palaeobotany.

Another small but important work appeared in 1954, *The mind and the eye, a study of the biologist's standpoint*. This short study gives in outline an account of the nature of biological research and of the thinking which a specialist uses when he considers the meaning of what he has observed. The work is a summary of the stages which a biologist like Mrs Arber has passed through on the road towards reality, but actually the work and thought described are very similar in those employed in pursuit of other branches of natural knowledge. The work of the biologist, if it is to lead men to any permanent result, must be continued to the stage of unearthing his basic assumptions and subjecting them to unprejudiced criticism.

Her last book *The manifold and the one*, published in 1957 by John Murray, was a philosophical essay, the outcome of the quiet contemplation which occupied her last years.

She died on 22 March 1960 in the Hope Nursing Home in Cambridge and was interred in the churchyard of the little church at Girton, close to the spot where her teacher and friend, Ethel Sargant, was buried.

I think it may be said that the passing of Agnes Arber ended a distinct period in the development of botany in England, a period in which comparative anatomy and morphology were regarded as the centre points of investigation. Later came the influence of physiological discoveries and the impact of ecological and cytological studies, but the careful investigation of plant structure by Mrs Arber and her contemporaries made a lasting contribution to botanical knowledge which is not likely to be superseded. Her writings on the philosophy of biological study, and her distinction between the phylogenetic speculation reached by typological arguments, and by the examination of plant form in the light of historical evidence, have still to become fully effective.

H. HAMSHAW THOMAS

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M Caullery

MAURICE JULES GASTON CORNEILLE CAULLERY

1868-1958

MAURICE JULES GASTON CORNEILLE CAULLERY was born at Bergues (Nord) on 5 September 1868 and died in his 90th year on 13 July 1958 in Paris. His father, formerly a French military officer, settled at Douai in 1877 and it was at the well-known Lycée de Douai that Caullery received his secondary education, specializing in the higher forms in mathematics and learning two modern languages, English and German. His aim was to secure a place at the École Polytechnique, but in 1887 he sat for highly competitive examinations both at the École Polytechnique* and the École Normale Supérieure† and was successful in both. However, his family responsibilities, caused by the sudden death of his father, compelled him to enter the École Normale, which offered a better prospect for more immediate security.

At about the same time Alfred Giard, Professor of Zoology at Lille University and formerly a 'normalien', returned to the École Normale as a lecturer in zoology. Caullery came under the spell of this highly stimulating and versatile naturalist and this led him to decide not to specialize in physics and mathematics, for which he was so well prepared, but to become a naturalist and to choose zoology as his main subject. Another factor which played an important part in his life was his meeting at the École with Félix Mesnil, also a pupil of Giard, who later became his brother-in-law and a life-long friend and co-worker.

Between 1888 and 1890 he obtained the degree of 'Licencié' in Mathematics, Physics and Natural History at the Sorbonne and in 1891 he and Mesnil, after passing their professional examinations of 'agrégation des sciences naturelles' at the École Normale, received the 'bourse de voyage' (travelling scholarship) of 2500 fr. each to visit several centres of biological research in Germany. Caullery spent some time at Heidelberg in the laboratory of Bütschli who had, only two years previously, completed his great work on the Protozoa for Bronn's *Thierreich.* At the cytological and protozoological laboratory of Richard Hertwig at Munich he met Boveri, who had just completed his important study of eggs of *Ascaris*, and there he also met the American cytologist, E. B. Wilson, and the young Norwegian, Johan Hjort, who became a distinguished oceanographer. At Jena he was well received by the veteran zoologist Ernst Haeckel, the contemporary and friend of Alfred Giard. In Leipzig he visited the very active laboratory of R. Leuckart and in Schultze's laboratory (Berlin) he met two young Privat Dozenten, E. Korschelt and K. Heider. He spent some time at Kiel in the laboratory of W. Flemming, famous for his work on cell division in *Salamandra*, and met his assistant F. Meves, who became a distinguished cytologist. In Amsterdam, on his way home, he met Max Weber, who, a few years later, led the oceanographic expedition of *Siboga*. It was in the Polychaete material collected by this expedition that Caullery many years later discovered the new invertebrate *Siboglinum*.

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* École Polytechnique, a military school training engineers for army, navy, mining and civil work. It counted among its pupils a number of distinguished mathematicians and physicists.

† École Normale Supérieure, a residential college for training professional teachers, leading to the qualifying examination of 'Agrégation'. The general education of its students leading to the title of Licencié, is obtained at the Sorbonne. Many distinguished professors of the Sorbonne and other universities of France were students of the École Normale and were often referred to as the 'normaliens'. It may be interesting to mention that in 1843 Pasteur contemplated preparing himself for both the École Polytechnique and the École Normale. He found, however, that the study of mathematics was too dry and exhausting and he entered the École Normale as a student, and later as a teacher. It was in a primitive laboratory of the École Normale that he made his first crystallographic discoveries (1846-1848).

Returning to Paris, Caullery rejoined Alfred Giard, who in the meantime had been appointed to the University chair of 'Évolution des Êtres Organisés', founded by the City of Paris, while Mesnil joined Elie Metchnikoff at the Pasteur Institute. At the Laboratoire d'Évolution, as well as at the Marine Zoological Station at Wimereux (near Boulogne) attached to the chair of 'Évolution', Caullery carried out his first important work on ascidians. This was the subject of his dissertation, for which he was awarded the degree of 'Doctor of Natural Sciences' in 1895. In 1896 he was appointed to a lectureship of zoology at Lyon and in 1901 to a Professorship of Zoology at Marseille. In 1903 he returned to the University of Paris and rejoined the Laboratoire d'Évolution as a Lecturer (Maître de Conférence); and in 1907 he became a Reader (Professeur Adjoint).

No proper appreciation of Caullery's research and teaching activities can be made without reference to the scientific atmosphere in France and other countries, especially during his scientific apprenticeship.

Between 1870 and 1900 the establishment of several marine zoological laboratories in France and elsewhere, extensive oceanographic explorations, the great interest in the phylogenetic relationships between different groups of animals and the early simple concept of ontogenetic recapitulation of the phylogenetic history of organisms, gave a great stimulus to the study of comparative anatomy and embryology, as well as to the discoveries of new forms and life histories of animals belonging primarily to the marine fauna. At about the same time cytologists unravelled the details of cell structure, the successive stages of cell division, maturation of gametes, the cytology of fertilization, and later the intraprotoplasmic structural elements. In France the experimental approach to the study of biological problems, inaugurated by the important investigations of Claude Bernard and Pasteur, was continued by their brilliant pupils. The battles of Pasteur with Pouchet, Liebig and Berthelot were still fresh in the minds of many biologists.

The chair of zoology at the Sorbonne and the directorship of the marine zoological laboratory at Roscoff attached to the chair were held by Yves Delage (1854-1920) who succeeded Lacaze-Duthiers, the founder of both the

Roscoff and the Banyuls marine laboratories. Several stimulating discussions on problems of general biological interest between Giard and Delage were closely followed by workers of Caullery's generation. In Paris important posts at the Sorbonne, Collège de France and the Muséum d'Histoire Naturelle and, to a lesser extent, in the provincial universities, were occupied by well-known workers in every field of biology.*

As Caullery admitted on several occasions, his scientific orientation was greatly influenced by his teacher Alfred Giard, who occupied in France a position not unlike that of Ernst Haeckel in Germany. In his numerous articles, lectures and discussions Giard was primarily responsible for making the work and views of Charles Darwin known and popular in France, not only among advanced research workers, but also among students and the educated public. It was at Giard's suggestion that his assistant A. Chappellier translated into French the two papers by Gregor Mendel (1865), which Giard published in his *Bulletin scientifique de la France et de la Belgique* (**46**, 371-419, 1907). It was also Giard who stimulated his pupil Marcel Landrieu to write an extensive book of 475 pages entitled *Lamarck, le fondateur du transformisme, sa vie et son oeuvre.* This important work was published by the Zoological Society of France in 1909 on the occasion of the centenary celebration of Lamarck's *Philosophie Zoologique* (1809), and the unveiling of his monument erected at the Jardin des Plantes, attached to the Muséum d'Histoire Naturelle, where Lamarck had worked and lectured. Unfortunately Giard died in 1908, just a year before this celebration.

In 1909 Caullery succeeded Giard (1846-1908) in the chair of 'Évolution des Êtres Organisés' and in the directorship of the Marine Zoological Station of Wimereux. The Laboratoire d'Évolution was housed at 3 rue d'Ulm in a small ramshackle building containing one large ground floor room for research workers, a small lecture room and a first floor, or rather an attic, divided by wooden partitions into a few cubicles, in which Caullery and his staff worked. The very small walled-in garden of the building was extensively used as a collecting ground of stranded representatives of terrestrial fauna. Dominating the view from all the windows of the building was the dome of the Panthéon. This laboratory building, in fact, which Giard had inherited from Paul Bert (the pupil and successor of Claude Bernard), was believed to have been the workshop of Soufflot, the architect of the Panthéon.† The building was very uncomfortable, inadequate for the work, and not altogether safe. Caullery spared no time and trouble to obtain more appropriate accommodation for his laboratory and, soon after the first world war, his efforts were rewarded. In 1923 the laboratory was transferred into a new luxurious home specially built for it in the Boulevard Raspail, where it now continues its active life under the direction of Professor P. P. Grassé.

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* The great activity of French biologists of this period is well summarized in Caullery's *Histoire des Sciences biologiques*, 1925 (see Bibliography).

† An enquiry kindly made by Professor Ch. Oberling, the head of the laboratory of experimental medicine which is at present housed in this building, disproved this belief. He found that the workshop of Soufflot occupied a small building standing nearer the Panthéon.

The marine zoological station founded by Giard in 1872, which was, after certain changes, inherited by Caullery, was a regular place of research in which members of his Paris laboratory used to meet visitors from other universities of France and other countries. The building, erected on the sand dunes, was not very well protected from the sea and it gave Caullery much anxiety about its safety. It was an excellent centre for training young workers who were closely supervised by Caullery. During the first world war the laboratory was temporarily used as an annexe to an Australian Hospital. Unfortunately during the last war, in 1942, it was razed to the ground by the German army. In 1944 Caullery published an account of its glorious history (1872-1942) in research and of its sad end. 'Ceux qui y ont vécu,' he wrote, 'n'auraient jamais songé à la façon dont elle devait périr et dont la brutalité dépasse infiniment les rigueurs inéluctables des éléments naturels, si impitoyables soient-ils. C'est pour une institution, une manière de mort au *champ d'honneur*.'

Much of Caullery's time was occupied by his personal research, by directing the work of his pupils and by his teaching. He lectured only on problems of general biological interest, each year being devoted to a different topic, such as sexuality, heredity, regeneration and grafting, experimental embryology, parasitism and symbiosis and other subjects. His lectures, based upon critical examinations of original investigations in each of these subjects, were very carefully prepared and delivered with great elegance. They reflected his intimate knowledge of the subject and were often enriched by the fruits of his personal contacts with workers whose contributions he discussed. Although the lectures were richly documented, they were never overburdened with superfluous details. The thoroughness of their preparation enabled him to publish several courses of his lectures in the form of books which had great influence upon the trends of biological research in France.

Caullery travelled extensively, either in charge of scientific missions, or as an exchange professor, or as a representative of French zoologists at the international congresses, and he never failed to establish friendly relationships with workers of different countries. In this he was greatly helped by his knowledge of languages and by his insatiable curiosity, as well as by his ability to obtain accurate information on the academic life and educational organization of the countries he visited. In return he gave equally valuable information on the universities and educational systems of France.

He was always interested in the teaching and research activities of the provincial universities, and deplored the excessive concentration of the intellectual forces in Paris. This centralization, which was primarily due to the lack of encouragement the provincial universities received, even from their often rich cities, had already been condemned by Pasteur* in the eloquent appeal in support of scientific activities in France, which he made soon after the disaster of 1870.

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* Pasteur, L. Pour l'avenir de la Science Française. Collection *À la lumière des textes oubliés*, 1947. Édit. 'Raisons d'Être', Paris.

Of special interest in this respect was the courageous Presidential address by Caullery to l'Académie des Sciences, delivered in January 1945, a few months after the liberation of France. He appealed to the members of this body to take a leading part in the reconstruction of scientific life in France and he gave it as his opinion that, however much one should respect the traditions of great institutions like the Académie des Sciences, it is dangerous 'to live in systematic immobility when everything around us undergoes transformation'. In view of the rapidly rising collective age of the members, he appealed to the Académie to attract younger and more active men who were fully aware of modern ideas and methods.* He also indicated that one of the factors which contributed to the unfortunate centralization of the intellectual forces in Paris was that most of the places in the Académie des Sciences were reserved for the residents of Paris, and he suggested that the number of the non-resident members should be increased.

For many years Caullery acted as the chief editor of the *Bulletin Biologique de la France et de la Belgique* (founded by Giard in 1872). This journal was intimately linked with his 'laboratoire d'Évolution' and with the marine laboratory of Wimereux and was the main channel for the publication of papers by his pupils on different topics of general and experimental biology. Caullery was also responsible for the organization of the first University Press in France based on what he had learned during visits he had made to the University Presses in England and the United States.

Zoologists who knew only Caullery's published work were impressed by the variety of problems it covered, the thoroughness of his observations, his erudition and his scholarly treatment of the historical background of problems he discussed. Those who worked in his laboratory and kept in touch with him were full of admiration for his great capacity for work and for his vitality and the mental alertness which he preserved until his last illness. His strong sense of responsibility towards the University, the Scientific Societies and the *Bulletin Biologique* . . . which he edited, left him very little time to spare for conversations with his students on topics lying outside their research problems. In fact, some students and visitors working at the Laboratoire d'Évolution thought him aloof and unapproachable. This impression was, however, soon dispelled whenever they joined him at the Marine Zoological Station (Wimereux). There, free from most of his duties, he worked with his students and visitors in the same room, led their frequent excursions for collecting the materials, talked to them freely about his experiences, his travels, different personalities he met and sometimes about their idiosyncrasies. This led to interesting general conversations well sprinkled with stimulating provocative and often humorous remarks, which often revealed to Caullery more about the personalities of his companions than did the discussion of their research. In his discussions of controversial problems with workers who did not share his views he usually appeared to be more interested in grasping their thoughts than in converting them to his own views. He always kept in contact with pupils who left him and followed their later activities with great interest. At one time most of the chairs of zoology in France were occupied by his former pupils.

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* His appeal reads as follows: 'Aussi, pous maintenir notre Compagnie en réelle harmonie avec la science qui s'élabore, est-il très désirable d'appeler parmi nous, aussi souvent que possible, des hommes encore jeunes, dont l'esprit est naturellement accordé aux idées et aux techniques les plus récentes et les plus fécondes.'

SCIENTIFIC WORK

The scientific work of Caullery covers, as can be seen from the appended Bibliography, a great variety of problems in zoology, embryology and general biology. Much of this work was published in collaboration with Félix Mesnil.* He had many workers in his laboratories both in Paris and Wimereux and, although he followed their work very closely, he practically never collaborated with them. In fact he never studied fresh-water or terrestrial animals, yet he encouraged his pupils to work on different aspects of their structures, life histories and their parasites. In later years he stimulated and encouraged much work in his laboratory on different problems of experimental zoology. In the brief summary of some of his work which follows, no distinction will be made between papers published by himself and those published in collaboration with Mesnil. The organisms in which he was particularly interested and to the study of which he gave much of his time and labour were parasitic Protozoa, Orthonectids, parasitic Crustacea, parasitic Turbellaria, Polychaetes and Ascidia.

Protozoa. Of great interest in this field was his discovery of several parasitic Sporozoa for which he created the order of Haplosporidia. He described both the schizogonic and sporogonic life cycles of these parasites. Large numbers of forms belonging to this order of Protozoa have been described by other workers, the chief interest of these organisms being that they form a link between Protozoa and some inferior Fungi. His last contribution to this subject was the general review of Haplosporidia published by him in 1953, when he was 85 years old, in Grassé's *Traité de Zoologie.*

While he was studying the life history of *Gonospora* parasitic in a polychaete worm, Caullery discovered the asexual reproduction of this gregarine which brought this form within the group of Schizogregarinae established by Léger. Of special interest was Caullery's observation that different stages in the development of this parasite were controlled by the life-history of the host. Under the name of *Siedleckia* he described a new micro-organism, showing some resemblance to Sporozoa. It was later shown by Chatton to belong to an interesting gregarine (Blastogregarina) which has some of the features of Coccidia.

His study of Actinomyxidia revealed for the first time the sexual cycle of these Sporozoa and this stimulated similar work on the Myxosporidia. Among his many other contributions to protozoology one can mention his discovery of the remarkable group of organisms, the Metchnikovellidae, which are intraprotoplasmic parasites of gregarines parasitic in Annelids. The systematic position of these strange organisms still remains undetermined.

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* An excellent biography of Félix Mesnil (1868-1938) by André Lwoff was published in *Ann. Inst. Pasteur*, **60**, 221, 1938.

Orthonectidae. This order was created by Giard (1877) for some peculiar organisms parasitic in certain echinoderms, Turbellaria, nemertines and annelids. They were known only in adult ciliated stages showing a marked sexual dimorphism. Caullery was attracted to the study of these organisms because, like the Dicyemidae, they showed a very simple structure, their bodies being composed of only two layers of cells without any cavity. In fact, van Beneden (1877) considered them to form a group occupying an intermediate position between Protozoa and Metazoa and created for them the phylum of Mesozoa, which is still accepted by some zoologists. This view was not shared by Caullery who postulated that these organisms originated from Metazoa, the simplification of their vegetative organs being due to their parasitic mode of life. While studying one of the forms of these organisms, *Rhopalura ophiocomae*, parasitic in an echinoderm *Amphiura*, Caullery unravelled the complete life history of these organisms. He found that both sexes, soon after leaving their host, are paired while swimming; the fertilized ova rapidly develop and liberate, within 24 hours, ciliated larvae which penetrate into the tissues of their ophiuroid host, giving rise to two kinds of plasmodia in which the germinal cells differentiate into the male or the female *Rhopalura*. There is thus an alternation of sexual and asexual cycles. Caullery was able to follow the successive stages of the development of this parasite within hosts experimentally infected with ciliated larvae. This was indeed one of the most elegant experimental studies of a complex life history of a parasitic organism.

Parasitic Crustacea. Among Caullery's numerous papers dealing with different aspects of parasitic Crustacea, his study of the Epicaridae, which are isopods parasitic in other Crustacea, is of special interest. In this group Caullery solved experimentally a problem which puzzled many workers, namely, the nature of the larval form described by F. Müller as *Microniscus* attached to the body of a copepod. He found that this larva is an intermediate stage between the free swimming epicaridian larva and the *Cryptoniscus* stage which is fixed to the body of other Crustacea and undergoes a complete metamorphosis with marked parasitic degeneration and the development of sexual glands.

He also made an extensive study of the Liriopsidae, isopods living as superparasites in *Rhizocephala*, which are themselves parasitic in crabs. Caullery discovered the complete disappearance of the central nervous system of these organisms and the complete degeneration of all their internal structures, the superparasite being reduced to a pulsating, incubating sac providing a good circulation of water around its eggs.

One of the most remarkable instances of parasitic adaptation discovered by Caullery was that of a copepod which he described under the name of *Xenocoeloma brumpti*, parasitic in the annelid *Polycirrus*. Here the organs of the host and of the parasite mutually interpenetrate each other to such an extent that it becomes very difficult indeed to find within the parasite a structure not containing some parts of the host. The copepod undergoes a degeneration of its organs so marked that no trace of its cephalic region or of its nervous system remains. It loses its ectoderm and its parietal muscles appear to be attached to an ectoderm provided by the host. The peritoneal endothelium of the host penetrates into the body of the parasite and covers its epithelio-muscular layer, which surrounds a cavity probably representing the remains of the alimentary tract of the parasite. Without, in fact, the discovery of the early nauplius stage characteristic of a copepod, the fully grown *Xenocoeloma* could scarcely be assigned to any known class of animals.

Polychaetes. Of particular interest was Caullery's study of polymorphism in sedentary Cirratulidae such as *Dodecarceria* which live in tubes or galleries bored in calcareous rocks, shells or *Lithothamnium.* In these polychaetes he found three morphologically recognizable forms: (*a*) sedentary forms, which are parthenogenetic and viviparous; (*b*) free swimming sexual forms, males and females, which leave the tubes and come to the surface to liberate their sexual products ('formes épitoques'), and (*c*) freely swimming parthenogenetic oviparous forms. The study of sedentary Polychaetes revealed that the formation of epitokous forms is a much more general phenomenon than had been previously recognized and that the famous *Palolo* belongs to this category. The transformations taking place in the internal organs of worms during the formation of epitokous forms are dominated by a sexual crisis not unlike those which accompany the metamorphosis of holometabolic insects.

In 1901, Caullery received some sedentary Polychaetes collected by the *Siboga* expedition (1899-1900) in the seas of the Malayan Archipelago. The results of his preliminary examinations of this material were published in a series of papers which appeared in the *Bulletin de la Société Zoologique de France* (1913 and 1914). It was not until several years after his retirement from the chair of 'Evolution . . . ' that he found time to complete the study of this material. The results of this investigation were published in the form of an extensive monograph of more than 225 quarto pages which appeared in 1944 when he was 76 years old.

Pogonophora. While he was examining the material collected by the Siboga expedition Caullery (1914) discovered a new type of invertebrate which he described under the name of *Siboglinum weberi.* He created for this form a new family, the *Siboglinidae.* Since this discovery was made, several forms of these organisms have been found in the Pacific and Indian oceans, in the Polar Basin and in the shore water of the Antarctic, and also in the Skagerrak. The extensive study of these organisms by A. V. Ivanov* revealed that *Siboglinum* is a representative of a new phylum of invertebrates, the Pogonophora, containing 5 families, 10 genera and about 45 species. According to

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* Ivanov, A. V. (1959) The Pogonophora and their systematic position. *Proceedings of the XVth International Congress of Zoology. London,* 1958, see pp. 345-348. Ivanov, A. V. (1960) Embranchement des Pogonophores in *Traité de Zoologie* edited by P. P. Grassé, vol. V. 1521-1622.

Ivanov (1959, 1960) they belong to the echinoderm-chordate stem and show close affinity to the phylum of Hemichorda, a conclusion which is in agreement with the early view put forward by Caullery (1914).

Ascidia. The comparative study of the organogenesis in the sexual and asexual development of Ascidia was the subject of Caullery's early contributions to zoology. He was one of the first workers who have shown that the same organ in oozoites and blastozoites may be derived from different germinal layers. Thus in *Distaplia* the nervous system and peribranchial cavity of the oozoites are of ectodermal origin, whereas in the blastozoites they are of endodermal origin. These somewhat perplexing results were confirmed and amplified by other workers who studied Ascidia and Bryozoa. However, according to Caullery, these findings would not necessarily contradict the general theory of germinal layers, if one assumes that during the asexual reproduction the cells from which the buds originate undergo a dedifferentiation. In fact the phenomenon of cellular dedifferentiation was observed much later during the metamorphosis of some holometabolic insects. Caullery also studied the complicated phenomena of histolysis and phagocytosis which accompany hibernation in some forms of Ascidia. He discovered the function of the epicardium in the sexual reproduction and was the first to explain the nature of the double larva of *Diplosoma* by showing that, while one of them is the oozoite provided with a cerebral vesicle and a sensory organ, the other is the blastozoite derived by a premature budding of the first larva during its early stages of development from an egg. These are only a few examples illustrating the scope of Caullery's study of different aspects of the comparative anatomy and embryology of Ascidia.

The mechanism of parasitic adaptation. Caullery's extensive study of parasitic adaptations influenced his views on the problems of the mechanism of adaptation and evolution of organisms. He did not, however, discuss these problems in his parasitological papers or even in his book on *Parasitism and symbiosis*, but he did discuss them in his general articles, lectures, in his book on evolution and especially in his scholarly and elegant work on the history of biological sciences in France (which will be referred to as the *History of biology*). In dealing with these problems he made a clear distinction between the concept of evolution and its mechanism, and also between the observed facts of adaptation and their mechanism. Like his teacher and predecessor Alfred Giard, he had a great admiration for Lamarck whom he rightly considered to be one of the foremost naturalists of his time and the founder of the concept of evolution. However, unlike some other admirers of Lamarck, he could not accept Lamarck's over-simplified explanation of the mechanism of evolution. Thus in referring to this he wrote: 'La conception lamarckienne de l'action modificatrice direct du milieu était trop simpliste et le monde qui lui correspondrait serait d'une variabilité déconcertante; le developpement de la physiologie, en particulier la conception du milieu interieur, telle que l'a établie Claude Bernard, nous montre d'ailleurs l'organisme compensant à chaque instant les variations exterieures' (*History of biology*, p. 271). Neither

could Caullery accept the claims of the supposed experimental demonstrations of the inheritance of acquired characters. With his usual perseverance he kept abreast of published works on different aspects of genetics and evolution. But, however much he appreciated the important advances made in these fields of research, he found it difficult to visualize that the mere combination of Mendelian mutations and natural selection could bring about the remarkable instances of parasitic adaptation whereby an organism could be modified to such an extent that, without knowledge of some short-lived early stages, even the phylum to which the organism belongs could scarcely be recognized. He expressed this view in several of his papers and lectures. However, his somewhat vague adherence to Lamarckism appears to be primarily an expression of his failure to find a satisfactory explanation of the remarkable adaptations he observed in nature. 'Ni le lamarckism originel,' he wrote in 1946, 'ni le darwinism complété par la théorie des mutations, ne peuvent offrir à notre esprit une image satisfaisante de la réalisation de la nature vivante que nous observons.' Moreover, in his repeated discussions of this subject he never failed to point out that his main object was to present the difficulties of the problem rather than to pretend to solve them.

He was well aware that the views he held on this subject were not popular among biologists, yet he did not hesitate to put them forward with his characteristic intellectual integrity and courage. He was, however, confident that further experimental work, observations in nature and clear thinking will bring us nearer to the proper understanding of the mechanism of adaptation and evolution of organisms.

Caullery's scientific work now forms an integral part of zoology and is often referred to without mentioning his name. Like the work of other great zoologists, it has reached the distinction of anonymity. With him disappeared our last link with what can be described as one of the heroic periods in the development of zoology.

DISTINCTIONS

On his retirement in 1939 from the chair of 'Évolution des Ètres Organisés', his 'Jubilée Scientifique' was celebrated by an impressive ceremony held at the Sorbonne presided over by the Rector of the University. Warm tributes were paid to him by his many colleagues and pupils.

He was honoured by academies, scientific societies and universities of different countries and received several decorations including that of the Grand Croix de la Légion d'Honneur and of the Commandeur de l'Ordre de Léopold. In 1928 he was elected Membre de l'Académie des Sciences, of which he became President in 1945. He was President of the Biological and of the Zoological Societies of France, Foreign Member of the Linnaean Society of London and Corresponding Member of the Zoological Society of London, which in 1947 awarded him the Linnaean Gold Medal. In 1948 he was elected Foreign Member of the Royal Society, a distinction which he greatly appreciated. The same year he was President of the XIIIth

International Congress of Zoology held in Paris. He was elected Honorary President of the XVth International Congress of Zoology held in London. He would have enjoyed nothing more than to take an active part in the work of this Congress, which was joined with the Darwin-Wallace Celebration, but he died three days before its opening meeting.

The last tribute paid to Caullery was the dedication of the volumes of the *Proceedings* of this Congress. 'À la mémorie de Maurice Jules Gaston Corneille Caullery' in the name of 'ses collègues les zoologistes du monde entier'.

In writing this biographical memoir I was greatly helped by Dr André Lwoff, For.Mem.R.S., who also kindly obtained for me, from Madame Caullery the photograph of Professor Caullery and information about his early life and education and, from their son M. Michel Caullery other valuable information used in this memoir.

D. Keilin

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C.A. Edwards

CHARLES ALFRED EDWARDS

1882-1960

Charles Alfred Edwards was born in Kitchener, Ontario, Canada, 78 years ago on 23 March 1882. Kitchener is an industrialized city some 80 miles west of Toronto.

His father, Samuel Edwards, and mother, Elizabeth Edwards, had four sons and two daughters. They moved to England when Edwards was only two years of age. Information about the early years of Charles Edwards is sparse but in 1898, at the age of 16, he was apprenticed to the Lancashire and Yorkshire Railway Works at Horwich, Lancashire. During his seven years with this firm he developed an interest in metals and alloys and spent some years in the Chemical Laboratory. This led to his being appointed in 1905 as assistant to Dr H. C. H. Carpenter (later Sir Harold Carpenter) in the Metallurgical Department of the National Physical Laboratory. Dr Edwards's first scientific publication followed two years later, jointly with Dr Carpenter, it being the 'Eighth Report to the Alloys Research Committee of the Institution of Mechanical Engineers'. This was an account of an extensive research on copper-aluminium alloys.

When Dr Carpenter was appointed to the Chair of Metallurgy at Manchester University, Dr Edwards joined him there as Lecturer. Dr Edwards's research interests turned towards steel with an accent on hardened steels and the theory of hardening by heat treatment. He published several scientific papers on this subject.

In 1908 Dr Edwards married Miss Florence Edith Roberts and in 1913 their son, Charles Harold, was born.

In 1910, Dr Edwards returned to industry taking the post of metallurgist with Bolchow, Vaughan & Company and Dorman, Long & Company of Middlesbrough. Here his interests were extended to the field of low-carbon steels and the experience he gained during the ensuing four years were to serve him in good stead in later years. Today, the iron and steel industry is highly familiar with the use of oxygen enrichment and it is of interest to recall that nearly fifty years ago Dr Edwards wrote scientific papers on the merits of oxygen enrichment in iron blast-furnace production.

When Professor Carpenter vacated the chair at Manchester in 1914 to become Professor of Metallurgy at the Royal School of Mines, London, Dr Edwards was appointed to the Manchester post. He was then aged 32.

It is noteworthy that Dr Edwards became a distinguished Professor of Metallurgy and acquired the well-deserved honour of Doctor of Science without full-time attendance at any university or technical college. Most assuredly he gained his widespread knowledge the 'hard way'. In May 1910 C. A. Edwards applied for the degree of M.Sc. under Ordinance 2(b) of Manchester University. He submitted four published papers and in due course was awarded the degree. In 1913, C. A. Edwards submitted twelve additional papers for the degree of D.Sc. and this was awarded by Manchester University.

During the period 1914-1920, Dr Edwards's interests tended to concentrate on the phenomenon of the hardening of steel and his researches led to the publication of some ten scientific papers bearing on this subject. Additionally, however, he found time to write a substantial book on *The physico-chemical properties of steel* which was a noteworthy contribution to the subject at that time. The research contributions made by Dr Edwards during this period were all the more remarkable as the accommodation for metallurgy at Manchester was poor indeed, namely, in the basement of the Chemistry Department. Furthermore, during the war period much effort was devoted by Dr Edwards and his staff colleagues on war work, largely the analysis of steels.

The Honours School in Metallurgy was only in its formative years, but from 1918 to 1920 the undergraduate and post-graduate strength grew considerably and included many people who are today most distinguished metallurgists. Such names as J. N. Greenwood, M. Cook, W. E. Atkins and L. E. Benson immediately come to mind.

Edwards was a pleasant, kindly man, deeply absorbed in his work but in no way absent minded. Young students and research workers were attracted by his personality and by his obvious and abundant enthusiasm for his subject. His zeal and energy seemed to infect those associated with him.

In 1920, Professor Edwards relinquished the chair at Manchester and became Professor of Metallurgy at the newly formed University College of Swansea. His first year's work was pursued under truly austere conditions in highly restricted accommodation provided by the Municipal Technical College, Swansea. It is particularly noteworthy that, notwithstanding the accommodation problems, three of Professor Edwards's colleagues accompanied him from Manchester—A. L. Norbury coming as Lecturer, A. J. Murphy (now Principal of the College of Aeronautics at Cranfield), and the late C. R. Austin. This was a tribute, surely, to the admiration and affection which Professor Edwards inspired in his colleagues.

In 1921, the accommodation problem at Swansea was relieved by the construction of temporary buildings adjacent to Singleton Abbey, the headquarters of the new college. Forty years later these 'temporary' buildings still house the metallurgy department! In those first years at Swansea the number of students in Edwards's department was inflated by men returning from service with the armed forces. These men were, of course, more mature than the average students but less readily amenable to the change to university life. It is particularly noteworthy that Edwards, by virtue of his friendly personality, eliminated difficulties and transmitted his enthusiasms for things scientific to these 'veterans'. It was Edwards's habit regularly to join in tea at 4 p.m. All in the department at the time joined in sitting on stools round a bench in the Assay Laboratory. It was always a 'free for all' with outrageous theories and explanations becoming tempered by argument and student problems resolved by free discussion.

Edwards quickly developed a deep-seated interest in the local industry of tin plate and black plate manufacture. Such esoteric terms as 'mottle' and 'stickers' became familiar in the department and researches under Edwards's direction flourished in such subjects as factors influencing the location of blow-holes in steel ingots, the deep drawing qualities of mild steel, grain growth in mild steel sheet during annealing, and the alloys of iron and tin. However, despite Edwards's deep-seated interest in the production problems of the metallurgical industries, nevertheless, theoretical explanations of the phenomena he encountered were never far from his thoughts. In the early thirties he delivered a series of fascinating lectures on the theory of alloys which, today, would be described as metal physics. In this field Edwards was 'before his time' and his work was never published.

When Edwards was appointed to the Chair of Metallurgy at the University College of Swansea in 1920 he was also appointed Vice-Principal and in 1926, when Professor Sibly resigned, he was appointed Principal whilst retaining the post of Professor of Metallurgy. Naturally the additional responsibilities reduced the time that Professor Edwards could give to metallurgical research and teaching. However, during the period from 1926 until his retirement in 1947 Edwards published some sixteen scientific papers mostly prepared jointly with post graduate research students.

Edwards was elected a Fellow of the Royal Society in 1930. This gave very great pleasure to all his friends both within and outside academic circles. Unhappily, only a few of those who attended the celebration party remain alive to recall this festive occasion. Seldom can a college of professors have exhibited the party spirit in such full measure.

As Principal at Swansea, Edwards spared no efforts to raise the status of the College and to improve the facilities for teaching and research. Noteworthy donations were made by local industrialists to help meet the cost of new buildings required by the rapidly growing student population.

On his retirement in 1947, Edwards by no means lost his interest in metallurgy. He became a Consultant to Messrs Guest, Keen & Nettlefolds and was regularly to be seen on the train going to his office in Cardiff.

Edwards was interested in his garden but his hobby could best be described as the reading of technical literature. This he pursued until his death on 29 March 1960.

L. B. Pfeil

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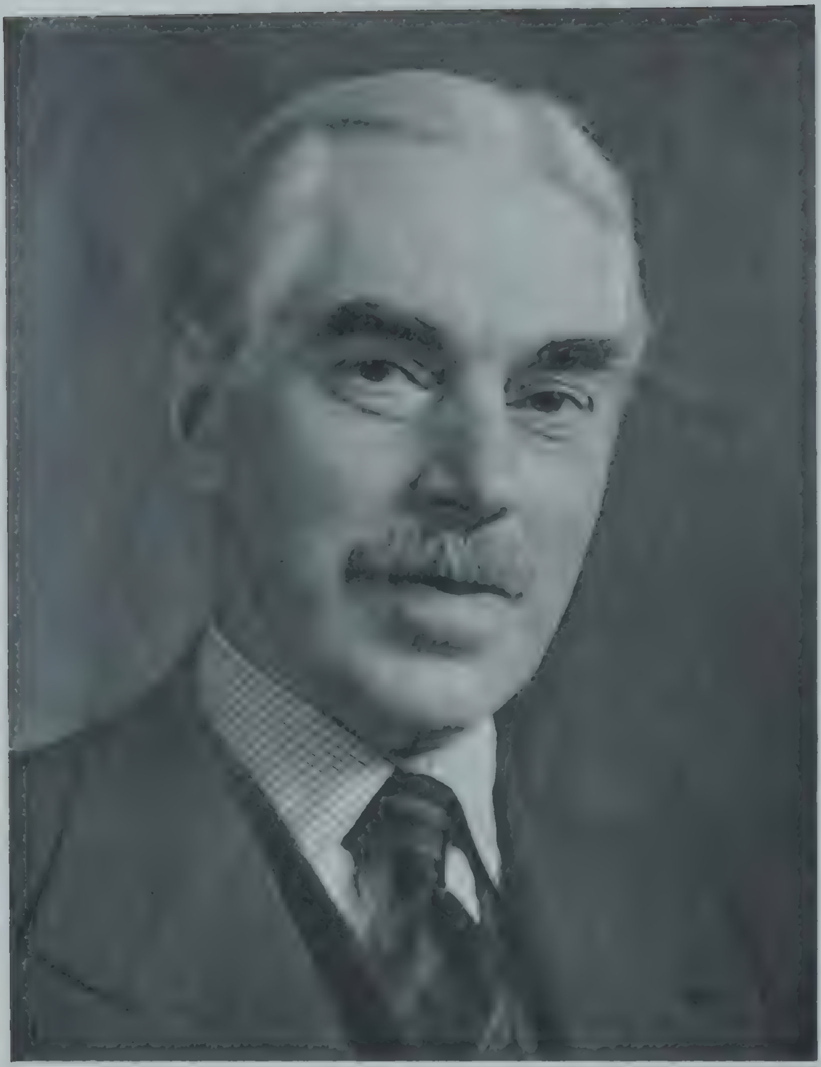


A. Egerton

ALFRED CHARLES GLYN EGERTON

1886-1959

> 'The world's wealth is its original men; by these and their works it is a world and not a waste; the memory and record of what men it bore—this is the sum of its strength, its sacred "property for ever".' *Thomas Carlyle*

THE SUBJECT of this memoir was of the essence of those men to whom Carlyle pays such a discerning tribute. Born towards the close of the Victorian era he grew to manhood and accomplished his life's work in one of the most tempestuous and critical periods of our history; his influence during his lifetime, although exerted unobtrusively, was immense; his many and varied public services and his scientific achievements are testimonies to a life governed by high ideals and by unremitting industry; and not the least of his services were those he gave to the Royal Society of which he was a Fellow for upwards of thirty years.

ALFRED CHARLES GLYN EGERTON, the fourth son of Colonel Sir Alfred Mordaunt Egerton, K.C.V.O., and the Hon. Mary Georgina Ormsby-Gore, eldest daughter of the 2nd Baron Harlech, was born on 11 October 1886 at Glyn Hall in Merioneth.

His family trace their descent from Sir Thomas Egerton (created Baron of Ellesmere in 1603) who was Lord Keeper to Queen Elizabeth and later Lord Chancellor to James I. His son John, whose name will always be associated with the production of Milton's *Comus*, became Earl of Bridgewater and was the great grandfather of the first Duke of Bridgewater. On the death of the third Duke the ducal title became extinct but the Earldom of Bridgewater passed to a cousin, John William Egerton, who became seventh Earl.

The eighth Earl, Francis Henry Egerton, was a man of some note both as a naturalist and an antiquarian. Educated at Eton and Christ Church, Oxford, he became a Fellow of All Souls and in 1781 was elected a Fellow of the Royal Society. On his death he left £8000 to the President of the Society to defray the cost of a work on Apologetics by a writer or writers to be selected. In accordance with his wishes the work was divided amongst eight authors whose collected works are known as the *Bridgewater Treatises*. One of them entitled 'Chemistry, meteorology and the functions of digestion, considered with reference to natural theology' by William Prout, M.D., is still known to the curious.

Alfred Egerton was a direct descendant of a cadet of the family of the second Earl who settled at Tatton Park, Knutsford. His father, who served in the Rifle Brigade and the Horse Guards, was Comptroller to the Duke of Connaught and both his parents were much at Court during the closing years of the reign of Queen Victoria. He, however, spent his early years at Glyn and Brogyntyn, the houses of his maternal grandfather, in typical country pursuits; he swam, rode and later became a useful shot and an enthusiastic fisherman. The love of the countryside and the knowledge of country lore acquired at so early an age never deserted him; in the rare moments of leisure accorded by an exceptionally busy life he seemed able instantly to recapture his youthful zest for rural pursuits and with gun or rod to lay aside for a time the more serious preoccupations of everyday life.

Long before he went to school and due to the influence of an unusually able governess, Miss Houlton, he began to take an interest in science and to show a gift for experimenting. Later, in 1900, when he entered Eton College, he was fortunate in finding a number of his contemporaries with similar interests, and a science master, Thomas Cunningham Porter, with the gift for arousing their enthusiasm and guiding their studies. Dr Porter was one of the outstanding science teachers of his time, combining an original mind with great personal charm and a keen sense of humour. Even with his long experience he must have looked upon 1900 as something of a vintage year, for in addition to Egerton there were amongst his pupils Robin Strutt, Thomas Merton and Julian Huxley, all of whom later distinguished themselves in various fields of science and became Fellows of the Royal Society. With the support of his friends Egerton founded the Eton College Scientific Society and took a leading part in its affairs. 'I am reading two papers on Radium', he wrote to his father in 1903 and in another letter of the same period: 'Yesterday we had an E.C.S.S. meeting and discussion on the best training for a scientific profession.'

This preoccupation with science was a cause of some bewilderment to his parents who had destined him for a naval career and failed to understand the fascination of a subject which was not then fashionable and about which they knew little or nothing. He was saved from this fate by the intervention of his aunt, Lucy Stanhope, wife of Edward Stanhope, Minister of War, who pleaded his cause with her brother in the strongest terms. 'He has,' she wrote in 1902, 'been so faithful to his evil smelling bottles and compounds as well as showing such sound and excellent abilities in mathematics and his other studies—that it would give me the very greatest pleasure to help him with whatever is necessary during the years of study now before him—and it really does seem a pity to waste and disregard a talent so strong as his.' . . . 'I wrote to Lord Rayleigh (who is an old friend) to ask his advice as to profitable openings and the best course of training—and send you his answer.'

The encouragement given by his aunt, the advice given by so eminent a figure as Lord Rayleigh, and his own strong determination eventually overcame the prejudices of his parents.

In the winter of 1903, 'Jack', as he was always known to his family circle and friends, spent his Christmas holidays with Robin Strutt and in a letter to his mother from Terling Place spoke in enthusiastic terms of his reception: 'Lord Rayleigh showed me his laboratories which used once upon a time to be a loft and cellars. He also showed me the apparatus and balance with which Argon was discovered.' . . . 'Lord Rayleigh allowed me to take a book out of his large selection of scientific works and I am reading a most interesting one by J. J. Thomson on Radioactivity chiefly.'

The impression made by this visit and by the kindly interest of his host confirmed his ardent desire for a scientific career. On leaving Eton he followed the course suggested by Rayleigh and in 1904 entered University College to read chemistry under Sir William Ramsay. Ramsay was then at the peak of his scientific activity. His work on the inert gases had been completed some years earlier and he was at this time, in collaboration with Frederick Soddy and Whytlaw Gray, embarking on a long series of researches into the properties and effects of the 'emanation' from radium.

Ramsay held views on the training of scientists which although not then generally accepted in academic circles were eminently successful in attracting young men of ability and imagination to work with him. In an oration delivered at University College on 6 June 1901 he had stated in uncompromising terms his ideas of the function of a university: ' . . . the duty of the professors, assistant professors, teachers and advanced students', he said, 'is to increase knowledge. That is the chief function of a university—to increase knowledge . . . no man can be a thoroughly good teacher who is not at the same time a learner; and in physical science that means an investigator.' In his laboratories students at all stages of training were kept in touch with the current researches and had an opportunity of appreciating the meticulous attention to detail and the refined techniques which characterized Ramsay's own experiments.

In this inspiring atmosphere Egerton served his apprenticeship and graduated with first-class honours in chemistry in 1908; he was elected President of the University College Chemical and Physical Society the following year. During his undergraduate days he formed a close friendship with Ramsay's son Willie and in an appreciation of his master written some years later he gives a charming glimpse of the family circle: 'I often used to be round at the house for supper or for a dinner party or for a dance. No one was more hospitable or had a more delightful strong and distinct character than Lady Ramsay whom I soon grew to love. I had three most pleasant separate weeks with the family in the summer holidays in Scotland . . . Sir William loved his boating and his bathing and sea fishing . . . I had many opportunities at these times to discuss with him ideas about atoms, the significance of atomic weights, about radium, etc. and used to sit and talk with him in the bow of the boat.'

Although his studies always had a first call on his time Egerton never became a recluse. His vacations periodically took him into a different world —a world mainly of country interests and sporting activities. He was a constant and welcome visitor at Glenferness and Kirtlington Park, the family homes of the Leslie-Melvilles; and there he fished, hunted and danced Highland reels. Whilst staying at Kirtlington Park or at his mother's house at Chilton near Thame he was also a visitor to Parmoor where the gifted family of the first Lord Parmoor provided an intellectual oasis and a closer contact with the great world of politics and government. Stafford Cripps was at this time a fellow student at University College and the friendship begun in the laboratory was later cemented by close family ties and by a strong community of interests.

Having completed his formal academic studies Egerton passed the summer of 1909 at Nancy University working under Professor Ganz and later hoped to spend a further period in Germany. He was, however, offered the appointment of Instructor at the Royal Military Academy, Woolwich, a post which he accepted with some reservations. In thanking the Commandant, Major Hildebrand, for the offer he did not hesitate to impose a condition. 'I want to make clear', he wrote, 'that unless I have facilities for research work it would be useless my accepting. My career depends chiefly on the quality of the research I can turn out, and not only that but my chief interest lies amongst problems of chemistry: and cut off from the necessary appliances to continue my research I should not long be happy.'

He stayed at Woolwich until 1913 instructing cadets in the elements of chemistry and devoting his spare time to research work on the oxides of nitrogen. It was a happy period of his life; he became engaged to the Hon. Ruth Cripps, only daughter of Lord Parmoor and sister of his friend Stafford, and their marriage took place in 1912; he was enjoying his work at Woolwich and he was making plans for an extended visit to Berlin. His first published paper entitled 'The divergence of the atomic weights of the lighter elements from whole numbers' was written whilst he was at University College and appeared in the *Transactions of the Chemical Society* for 1909. Three other papers on 'The vapour pressure of nitrogen peroxide' and 'The heat test of explosives' were published between 1913 and 1914 and described work carried out at Woolwich.

During this time Egerton kept in close touch with his old teacher Ramsay and when the latter retired in 1912, frequently assisted him in experimental work at his home in Chester Terrace. Ramsay was interested in the possibility of the transmutation of the elements and some observations by his colleagues Collie and Patterson on the appearance of helium and neon in hydrogen through which an electric discharge had passed, appeared to him to be of considerable significance.

In 1913 Egerton proceeded to Berlin where he had been offered a place in Nernst's laboratory. Here, at the suggestion of Ramsay, he carried out a very careful repetition of Collie and Patterson's work but failed to confirm their results. His experiments were subsequently described in the *Proceedings of the Royal Society* (1915). He also commenced work on the vapour pressure of metals but owing to the outbreak of the 1914 war was unable to finish it during his stay in Berlin. Amongst the students working with Nernst at this time was F. A. Lindemann (later Lord Cherwell), and the two Englishmen formed a friendship which was continued later at Oxford and lasted until Lindemann's death in 1957.

Nernst was quick to appreciate the quality and integrity of his new assistant and testified his regard by many acts of kindness and in particular by the pains he took at the outbreak of war to ensure that Egerton and his wife should not be detained in Germany; the young couple arrived back in England on the day before war was declared.

Although he wished to join the armed forces and commenced training with the Coldstream Guards, Egerton was almost at once directed to the Department of Explosives Supply of the Ministry of Munitions where he worked on a variety of urgent problems connected with explosive manufacture. Later, under the leadership of K. B. Quinan, he took part in the design and erection of the great National Explosives Factories which became necessary to meet the munitions crisis of the mid-war years. Quinan was a forceful personality, an outstanding chemical engineer and a man of great drive. The team of scientists and engineers he gathered around him were inspired by his fervour and enthusiasm, and their achievements under every kind of difficulty and discouragement contributed in no small measure to our final victory. In later years, when Egerton was head of a large department of chemical engineering, he showed by his grasp and understanding of the problems of large-scale production the profound influence of this wartime experience.

Another war activity was in connexion with the application of science to naval warfare. Prior to 1914 the Admiralty had possessed no adequate research organization and depended almost entirely upon the experimental work carried out by its contractors. At the outbreak of war it soon became apparent that there was an urgent need for some machinery for dealing with the large volume of ideas and inventions from outside sources which were flooding some of its departments. Criticisms were raised in Parliament and as a result Mr Balfour in 1915 set up a Board of Inventions and Research (B.I.R.) consisting primarily of a panel of scientific advisers and an executive committee under the presidency of Lord Fisher.

This improvised measure was not entirely satisfactory and in 1917 Sir Eric Geddes, First Lord of the Admiralty, appointed a small committee consisting of Sir R. Sothern Holland, Sir H. Ross Skinner and Egerton to examine its activities and to make recommendations. This was the first of many such enquiries in which Egerton was to be involved during the next forty years and he brought to it a mature judgement and a characteristic thoroughness. The following note appears in his diary: 'One of the most interesting men, among the many we met in connexion with our work at B.I.R. was Lord Fisher. He was president of the Board, but his interest lay still in Naval Strategy rather than applied science. He was always to be found near a chart. He said his walk was up and down one side of Berkeley

Square (where he lived), it was like aboard ship—he could end his walk when he liked and needn't go a long way back home. He was at his best entertaining us at a lunch to meet Sir J. J. Thomson, Sir G. Beelby and Sir C. Parsons. His stories were innumerable and his wit sparkled like the "bubbly sunshine" he provided us.' In its final report the Committee recommended the abolition of the Board of Inventions and Research and the setting up of a Central Establishment of Research.

Towards the close of 1917 Egerton was busy studying the problems of synthetic ammonia production and preparing a series of papers outlining a definite scheme of post-war development. He was, in common with many of his colleagues, beginning to feel the physical and mental strain of the war years. He was shocked by the great wastage of life and material resources; the loss of his two elder brothers in France filled him with sorrow; and in his diary there is evidence of a feeling of despondency. 'In many ways', he wrote, 'one would be happier if one's lot were cast amongst those who are in the melting pot of France, who have no thought for the morrow, but a single common purpose and cannot see or realize the root cause for that purpose.'

Fortunately, he was living at this time at his father-in-law's house in Wilton Crescent and was brought into close touch with a group of distinguished men and women who under the leadership of Lord Parmoor, were advocating the formation of a League of Nations. It was a crusade founded upon Christian principles and with a wide appeal to thinking people; it offered in its promise of ensuring international peace and good-will some compensation for the tragic losses of the war. President Wilson regarded it favourably and, with growing support from influential quarters, it did much to arouse a more optimistic spirit amongst all classes.

In January 1919 at the suggestion of Dr F. A. Freeth, Lord Moulton, Director General of Explosive Supplies, organized the visit of an inter-allied Mission to the German chemical factories in the occupied zone to investigate their production of munitions of war. Egerton was a member of the Mission which included British, American, French, Belgian and Italian representatives and was led by Harold Hartley, the Controller of the Chemical Warfare Department of the Ministry of Munitions. The task of the Mission was by no means easy as can be seen from the references to it in the autobiography of Carl Duisberg, the head of the Bayer works at Leverkusen, the first to be visited. The success of the Mission owed much to Egerton's planning of the visits to the different factories and to his tact in dealing with difficult situations, helped by his fluent knowledge of European languages. His own special assignment was to examine the German nitrogen industry which by exploiting Haber's process for the synthesis of ammonia, had made such a significant contribution to their war effort. The findings of the Mission were published as a White Paper which brought home to Parliament the need to strengthen the British chemical industry. The Report itself provided the technical and economic data on which the Government could frame a policy.

The coming of peace and the re-deployment of the scientists who for five years had been engaged almost entirely on war work posed many difficult personal problems. Egerton was anxious to resume his researches without delay and on the advice of his friends Henry Tizard and F. A. Lindemann, accepted an invitation to work in the Clarendon Laboratory at Oxford. Here he stayed for some seventeen years, succeeding Tizard as Reader in Thermodynamics in 1923, and laying the foundations of the long series of investigations into the phenomena of combustion which continued without a break until his death in 1959.

The old Clarendon Laboratory, built in 1872, reflected in many ways the spirit of an age in which science was held in slight esteem. Its design was by modern standards inconvenient and many of its services rudimentary. As a newcomer Egerton was relegated to the cellars where the handicaps of bad light and ventilation and the presence in summer of clouds of mosquitoes were only partially compensated for by a certain uniformity of temperature and freedom from interruption. In these somewhat austere surroundings he was perfectly happy. His training under Ramsay had made him self-reliant and resourceful; he had a natural genius for improvisation, and much of his best work was carried out with the simplest of apparatus combined, however, with exceptional manipulative skill. In 1919 he resumed the investigation he had started in Nernst's laboratory on the vapour pressure of metals and some idea of his intense application is given by the fact that in 1923 he wrote no less than seven papers on the subject. By 1935 when he terminated this research, he had published extensive data on the vapour pressures, chemical constants, latent heat of vaporization and the temperature coefficients of the specific heats of cadmium, cadmium-zinc alloys, sodium, potassium, lead, magnesium, thallium and zinc.

In 1924 he first began to take an interest in combustion and was soon absorbed by the many complex problems it presented; some years later he was also asked by the British Electrical Research Association to supervise a programme of work on the saturation pressure and enthalpy of steam, a continuation of the pioneering experiments of H. L. Callendar. The results of this investigation are incorporated in the present *International steam tables.*

His work was attracting attention and with an increasing number of students wishing to study under him, the question of accommodation and a more generous financial provision became urgent. He had at the close of the war accepted the appointment of consultant to the Central Mining Corporation and he now found it necessary to devote a considerable proportion of his fees to the purchase of apparatus and instruments for his researches. In a memorandum written in 1934 he drew attention to the difficulties under which he was working and outlined a proposal for establishing a separate laboratory or Institute of Thermodynamics at Oxford. 'I think as time goes on', he wrote, 'the teaching of physics will be more and more related to the basic principles of the subject rather than to the older divisions of the subject, 'heat, light, sound, mechanics and electricity. The principles I refer to are

Dynamics and Hydrodynamics, Thermodynamics, Electro-magnetic Theory and Statistics. So that from this point of view, the establishment of a Thermodynamical Laboratory need not be feared by the University. There is bound to be overlapping with the Physics, Engineering and Chemical Departments, for Thermodynamics, being a basic science, enters into all branches of natural philosophy.' 'It is suggested that further advantage might be gained if there was more opportunity for students to learn methods of temperature measurement, of pyrometry, calorimetric measurement, and of other thermodynamical experimental methods, which would be possible if a laboratory were available.'

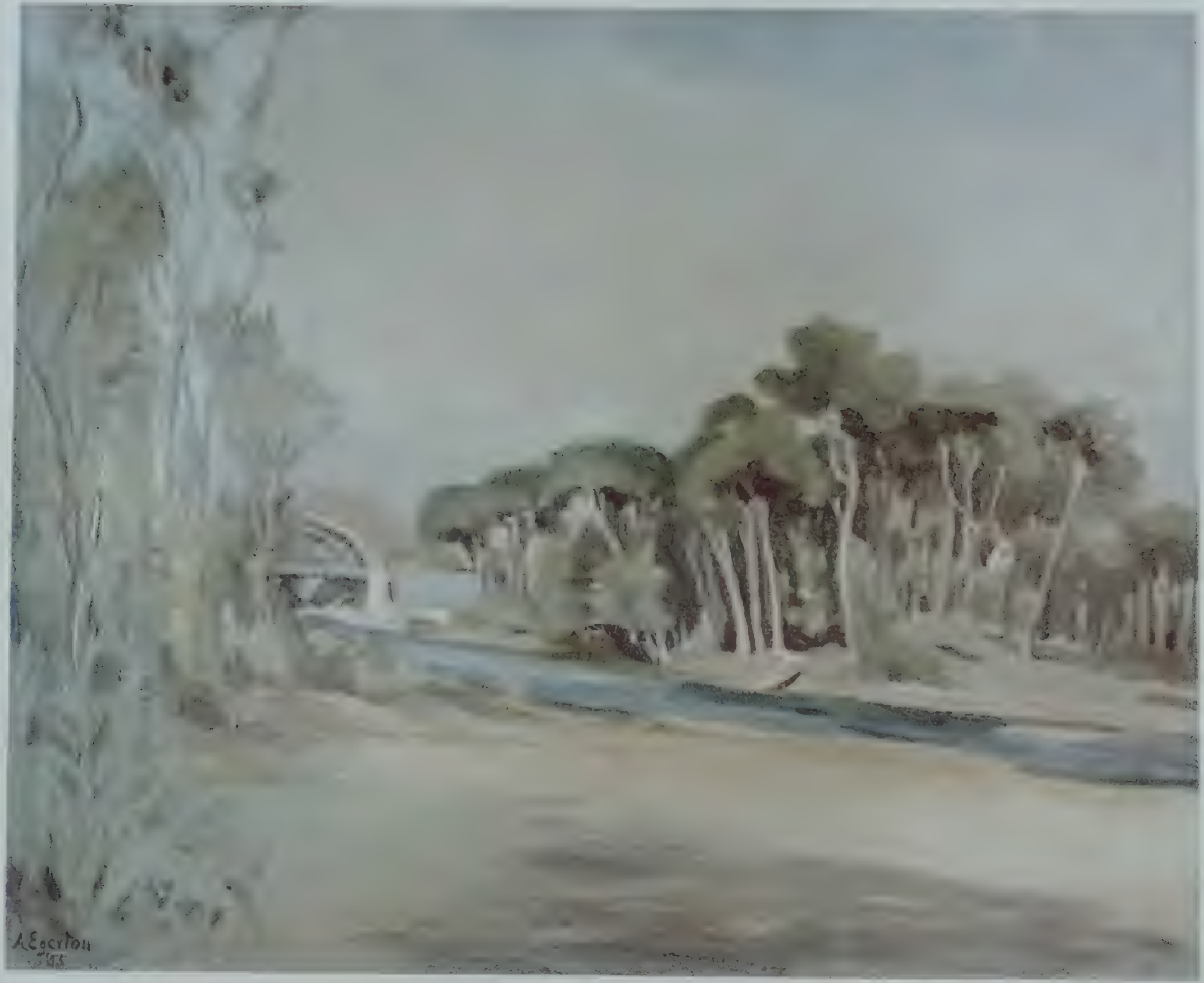
The arduous scientific work at Oxford was never allowed to submerge Egerton's wider interests or to curb the natural kindliness of his character. His home life was a singularly happy one; at the close of the war he and his wife had adopted Francis, the posthumous younger son of his brother Louis who had been killed in France in 1917, and this addition to their household was a constant source of delight and interest to them. The hospitality of their home in Linton Road was enjoyed by a wide circle of friends distinguished in every walk of life. Here Egerton's students could listen to Einstein playing the violin or watch their host and Lindemann engaged in a close game of squash. Talk ranged over the whole field of human affairs and they were encouraged to take an active part in the discussions of their eminent seniors. Nor will they forget the charm of their hostess and the sympathetic interest she took in their personal affairs.

Egerton was particularly happy in his relations with his students. Always accessible, patient and kindly, he spared no pains in helping to overcome their difficulties; his attitude was that of a collaborator rather than a teacher and he was completely free from any suggestion of pomposity or superiority. His laboratory was a 'happy ship' in which friendliness, enthusiasm and optimism combined to produce a bracing atmosphere eminently favourable for original work. Even the most unimaginative of students could be stimulated to creative flights whilst those of exceptional ability found ample opportunities for further development.

Egerton was never a ready speaker or debater but his command of his subject, his patent sincerity and his singleness of purpose always ensured an attentive and sympathetic audience. He was perhaps at his best as a chairman, and in this capacity he served on many important committees; his industry was astonishing; he read and mastered every committee paper, took great pains with the preparation of agendas and directed proceedings with unfailing courtesy but commendable firmness. As chairman he generally undertook the drafting of reports and on paper his style was clear, concise and business-like.

In 1925, at the age of 39 years, he was elected to the Fellowship of the Society.

His last few years at Oxford were crowded and eventful; his researches on combustion were breaking new ground and were provoking lively and fruitful



RIVER SIAGNE, PROVENCE

A. Egerton

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discussions with other workers in the field. He was also serving on the Scientific and Advisory Committee of D.S.I.R., the Fuel Research Board, the Heating and Ventilating Research Committee, the Engine Committee of the Aeronautical Research Council, the Water Pollution Board and the Advisory Committee of the London and Midland Railway; and from 1931-33 he was a member of Council of the Royal Society.

In the midst of these multifarious activities his brief periods of leisure were mainly spent abroad in Austria, Switzerland, Italy and France. He had early developed an interest in painting, one that grew stronger with the years and became a continuing source of pleasure and refreshment to him. From every visit abroad or holiday at home he now returned with sketches and finished paintings in water colours which bore witness to his artistic talent and his lively sense of colour and form. After his retirement in 1953 he took to painting in oils and his landscapes depicting scenes in Provence and the Alpes-Maritimes reflect much of the beauty of these favoured regions. As a French critic described them: 'Elles frissonnent de gaie lumière; elles se parent de couleurs tendres, de couleurs vives. Elles respirent le plein air où elles sont nées.' An exhibition of his pictures at the Chenil Galleries in 1957 surprised even his friends by their versatility and charm. One example from the exhibition now hangs in the apartments of the Academy of Science in Moscow, and another in the Egerton Library at the Imperial College of Science. One of his paintings is reproduced here as a coloured plate.

In 1936 Egerton was invited to occupy the Chair of Chemical Technology at the Imperial College of Science, in succession to William A. Bone, F.R.S., who was then due to retire. The decision to leave Oxford was not an easy one and after the fashion of the first Lord Burleigh, he drew up a schedule of the advantages and disadvantages of such a move. London scored marks on the grounds of convenience and cultural activities, Oxford on those of peace and the beauty of its gardens and buildings. Against the items 'health' and 'expense' are placed question marks. The opportunity of widening his interests and carrying into effect his ideas on postgraduate training in a department under his sole control tipped the scales and he decided to accept.

The Department of Chemical Technology and Applied Physical Chemistry had been founded in 1912 and under the direction of Professor Bone, had earned a high reputation as a centre for the training of chemical technologists and chemical engineers. Professor J. W. Hinchley had, until his death in 1931, been in charge of the chemical engineering, and Professor G. I. Finch, F.R.S., of the applied physical chemistry sections. Active research was going on in the fields of combustion, coal science, blast furnace technology and electron diffraction. The workshop facilities were exceptionally good and there were plans for a considerable extension of the laboratories.

Egerton set about reorganizing the Department with vigour. He gave a much greater degree of responsibility to senior members of his staff, established a departmental library and stores and was very successful in raising funds for research.

His experience in the first world war and his subsequent contacts with industry had brought home to him the importance of chemical engineering and the need for a more thorough training of chemical engineers than that provided by a one-year postgraduate course. He, therefore, supported strongly a proposal for inaugurating undergraduate courses in the subject and was successful in establishing one in his department in 1938. Before the outbreak of the second world war he had resumed his researches on combustion and had begun work on the thermodynamic properties of multi-component hydrocarbon systems. He had also set up a laboratory for work at low temperatures.

In 1938 he was appointed Physical Secretary of the Royal Society and from that date until 1948, when his period of office expired, he was much occupied with its affairs.

The summer of 1938 was overshadowed by the threat of war. Egerton and the writer were attending a conference at Nancy when the French army mobilized; there was a feeling of calamity in the air and driving back through the champagne country it was evident that the rural population had little heart for the coming struggle. Although hostilities were postponed for a time preparations were made both in England and France for a war in which the civil population would undoubtedly be involved. Plans for the evacuation of the College were drawn up but Egerton was very insistent that his staff and research students should not be dispersed. His earlier experience had shown the value of trained teams of workers capable of dealing expeditiously with urgent practical problems. His representations were successful and his Department during the years 1939-45 acted effectively as a focal point for much important government work.

In view of recent developments in the gas industry special mention may be made of his pioneer experiments carried out during the early years of the war on the use of liquefied methane as an alternative fuel to petrol. There are large quantities of relatively pure methane gas issuing from bore holes in various parts of the country and a less pure gas is produced from the cracking processes in oil refineries and from the anaerobic fermentation of sewage sludge. Methane liquefies at $-161\cdot5^\circ$ C, the liquid having a density about two-thirds that of petrol. As a fuel it has many desirable qualities; it has a large calorific value, burns smoothly in air and when used in an internal combustion engine explodes without 'knock' even at high compression ratios. The technical problems connected with its use are concerned with the design of a suitable liquefaction plant, fuel tanks sufficiently well insulated to avoid excessive losses by evaporation and mechanically strong enough to withstand the rough treatment to which road vehicles are subjected. A special design of carburettor is necessary for the engine and there are minor problems associated with re-fuelling and with large-scale storage. All these problems were satisfactorily solved and extensive practical trials were carried out successfully on a municipal motor bus in service in the Midlands. The work is described in a paper in the *Journal of the Institute of Fuel*, 1945.

Another section of the Department undertook researches connected with the improvement of fire-raising equipment and the complementary problem of fire protection. A member of its staff was also responsible for the development of equipment employed in subversive warfare in enemy-occupied territory.

In addition to these activities Egerton was a member of the War Cabinet Scientific Advisory Committee, Chairman of the Fuel and Propulsion Committee of the Admiralty, and an *ex-officio* member of innumerable committees connected with the work of the Society.

In the summer of 1942 he was sent to the United States to re-organize the work of the British Central Scientific Office and to endeavour to improve the scientific liaison between London and Washington. This was a task for which he was by temperament well fitted. During his three months' stay he visited many of the American and Canadian service establishments and had numerous discussions with leading American scientists including J. B. Conant, Carroll Wilson and Vannevar Bush. He found the atmosphere friendly and co-operative and he had no difficulty in arranging for a much freer exchange of information than had hitherto taken place.

He appears to have interpreted his terms of reference in a liberal sense, for amongst his many engagements he visited a women's penitentiary in W. Virginia and gave an address to 200 black and 400 white prisoners.

On his return he was much occupied with the work of the Heating and Ventilating Committee of which he was Chairman and with the various activities of the Royal Society.

The *Report of the Heating and Ventilating Committee*, published in 1945, is one of the significant contributions to a subject of the greatest national importance; it is based upon a detailed scientific study of all the factors concerned with air conditioning and lays down standards which have proved of immense value in the design and construction of public and domestic buildings and in the promotion of fuel economy.

Egerton received the honour of knighthood in 1943.

The immediate post-war years were marked by profound political, economic and social changes in all parts of the world. Industries which had been mobilized for war work had to change rapidly to peace-time production; there were universal shortages of raw material, food, fuel and manufactured goods; the setting up in this country of the 'welfare state' with the nationalization of the main sources of energy and transport was making heavy demands on the nation's technical and scientific manpower; and there was an inflationary tendency which for many years was to lead to a succession of monetary crises. It was a time when cool judgement, administrative ability and bold initiative were badly needed.

Although Egerton had a deep interest in the problems of government and a wide experience of the internal working of the Civil Service he had no political ambitions. His services as a scientist were always at the disposal of the Government and, when asked, his advice was given impartially to either political party. From the scientific and technological point of view he was particularly interested in the long-term planning of the nationalized coal, gas and electricity industries and as Chairman of the Scientific Advisory Council of the Ministry of Fuel and Power from 1948-53 he was able to make his influence felt at policy-making levels. He gave much thought to the underlying principles of man's use of energy and his reflections upon this important subject were admirably summarized in a lecture entitled 'Civilisation and the use of energy' which he delivered at the Birmingham Meeting of the British Association in 1951. In his discourse he endeavoured to illustrate human activities in relation to their energy content by means of the diagram reproduced here. The following quotation will serve to explain its bases:

> 'In the centre of the diagram there is a circle representing the sun, the main source of the energy which man uses. From this centre a line is drawn upwards to a position representing the conversion of the sun's energy into all the cultivated products, including the products of natural plant growth and the animals, too, which obtain their food

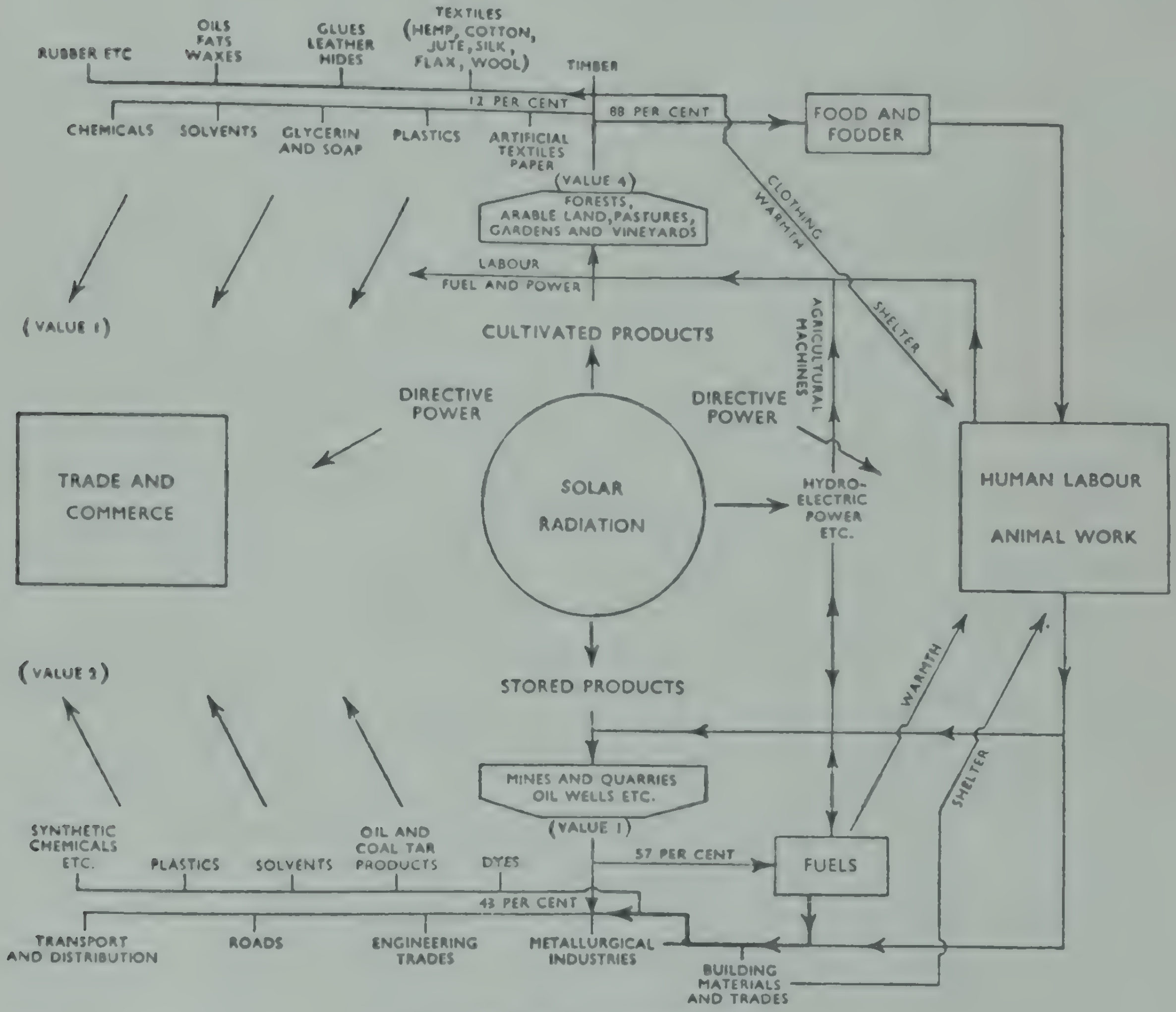
RUBBER ETC
OILS FATS WAXES
GLUES LEATHER HIDES
TEXTILES (HEMP, COTTON, JUTE, SILK, FLAX, WOOL)
TIMBER
12 PER CENT
88 PER CENT
FOOD AND FODDER
CHEMICALS
SOLVENTS
GLYCERIN AND SOAP
PLASTICS
ARTIFICIAL TEXTILES PAPER
(VALUE 4)
FORESTS, ARABLE LAND, PASTURES, GARDENS AND VINEYARDS
CLOTHING
WARMTH
LABOUR
FUEL AND POWER
(VALUE 1)
CULTIVATED PRODUCTS
AGRICULTURAL MACHINES
SHELTER
DIRECTIVE POWER
DIRECTIVE POWER
TRADE AND COMMERCE
SOLAR RADIATION
HYDRO-ELECTRIC POWER ETC.
HUMAN LABOUR
ANIMAL WORK
(VALUE 2)
STORED PRODUCTS
WARMTH
SHELTER
MINES AND QUARRIES OIL WELLS ETC.
(VALUE 1)
SYNTHETIC CHEMICALS ETC.
PLASTICS
SOLVENTS
OIL AND COAL TAR PRODUCTS
DYES
57 PER CENT
FUELS
43 PER CENT
TRANSPORT AND DISTRIBUTION
ROADS
ENGINEERING TRADES
METALLURGICAL INDUSTRIES
BUILDING MATERIALS AND TRADES

Diagram of Human Activities.

from the plants. In fact, not only man's food (grain, fruit, etc.) and the fodder for his animals, but the products timber and wood; cotton, flax, hemp and rubber; cattle and sheep (and pigs and poultry and fish); wools and hides, silk, oils, fats and waxes—the whole gamut of activities which emerge from forestry and husbandry and the cultivation of the soil.

'Then there is a line drawn downwards to a position representing the quest for the products stored from the sun's energy in past ages—the coal mines, the oil wells and included in this (though not the result of the solar radiation) are the quarries, the metalliferous mines and the gravel and clay pits and so on.

'To the left of the upper part of the diagram there are two lines, the top representing the products of cultivation, husbandry and forestry which form the basis of subsidiary industries. Along the lower line are specified some of these industries.

'Similarly for the lower part of the diagram, there are also two lines: the top line representing various products from the coal and oil, and the second representing some of the general activities depending on the acquisition of the products of mineral resources. The region on the extreme left is supposed to represent trade and commerce connected with the products relating to all these activities.

'To the right of the diagram there is a corresponding region representing human labour and the work of man's animal helps, the muscular power which is maintained by the food and the fodder without which none of the activity occurs. Lines are drawn to show where this power operates, in the fields, or the mines. Man's main needs are food, shelter, warmth and clothing. Lines are drawn indicating from where these needs are met. There is also need for water which he gets for very little, and for air which he gets for nothing.

'At the bottom right of the diagram, is reserved a place for fuel (got from the mines and oil wells). There is a thick line here so as to indicate that the energy of the fuel not only provides warmth but supplements the muscular energy of man and his animals. There is a line from the centre to the right to indicate that some energy is got from the unstored solar radiation, appearing as hydroelectric power or wind power . . .

'. . . figures were placed on the diagram to give an idea of the scale of these activities. The value in terms of gold of all upper half activities was estimated to be about four times the total of the lower, and of these activities 88 per cent. was in the production of fuel and fodder, and the rest in the growing of cotton, hemp and all the rest. The production of food was in fact shown as man's main activity. These figures are taken from Sir Harold Hartley's Mather lecture.

'Of the other fifth representing all the activities in the lower half of the diagram 57 per cent. was the proportionate value of all those activities relating to the winning and treatment of the fuels, leaving 43 per cent. for the values of all the other activities along that line.

FOOD TECHNOLOGICAL RESEAR
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> 'The value of the products, however, of the lower half activities—the manufactured and engineering goods—was estimated to be double the value coming from the industries along the upper lines from the cultivated products.
>
> 'Finally there are arrows to represent that people are employed not only in activities involving muscular effort, but also through offices, banks and government departments, etc., in administration and the direction of affairs. A considerable fraction too of labour employed in modern industry and modern agriculture is of course not occupied in exerting muscular power, but rather in exercising skill in controlling machinery which is exerting power, and in directing energy so that it may be used effectively.'

Another subject which engaged his attention at this time was that of disseminating scientific information and he devoted much thought and time to the organization of the Royal Society Scientific Information Conference which was held in London in 1948. One of the recommendations of the Conference was that the Society should set up a standing committee to implement its findings and this was given immediate effect, Egerton being appointed its Chairman. He later gave a full account of its work in the Aldred Lecture delivered before the Royal Society of Arts in 1952.

This was his last major work as Secretary of the Society. His period of office had been a strenuous one marked by events of the highest importance in the history of science and of the Society. In an address given at the Memorial Service to Egerton in 1959, Professor A. V. Hill, F.R.S., who was Biological Secretary from 1935-45, paid a moving tribute to his colleague: 'During the years that Jack and I served together at the Royal Society, 1938-45, and in the unusual tasks we had to undertake in those critical times, I profited greatly by his resolute co-operation, and his invincible obstinacy when he knew he was right. When he retired in 1948 from the Secretaryship he wrote me, in reply to a letter of mine, "I feel very contented that the Society is in such good fettle and one of the mainstays of freedom." That could sound a grandiose claim, but not from him; for the things he worked for are part of freedom, and his delicate understanding and obstinate resolution helped to achieve them.'

His release from the onerous duties attached to the Secretaryship brought no diminution in his many activities. The School of Combustion Research which he had created at Oxford and transplanted to London was in full vigour. Between 1948 and the time of his retirement in 1952 he published upwards of seventeen scientific papers of which no less than ten appeared in the *Proceedings of the Royal Society*. He was also much concerned with the problem of the training of scientists and technologists. As a member of the Barlow Committee on Scientific Manpower he had been impressed by the growing demand of industry for scientists and technologists of the highest calibre and by the lack of facilities at the schools and universities for meeting it. He exerted all his influence to improve the teaching of science in the schools and to obtain a more adequate provision for postgraduate work at the universities. He also interested himself in the promotion of liberal studies at the Imperial College of Science and was for several years Chairman of its General Studies Committee. He was for many years a member of the Governing Bodies of Charterhouse and of Winchester College and from 1949-59 was Director of the Salters' Institute of Industrial Chemistry.

But the subject closest to his heart was the future of the under-developed countries. He had been much impressed by the inaugural speech of President Truman in 1949 which contained a powerful appeal on their behalf. 'More than half the people of the world', said the President, 'are living in conditions approaching misery. Their food is inadequate. They are victims of disease. Their life is primitive and stagnant . . . For the first time in history, we possess the knowledge and the skill to relieve the suffering of these people.' Egerton's humanity and reason alike were enlisted in a cause to which he felt this country had a valuable contribution to make, and at an age when most people are contemplating retirement and repose, he undertook many strenuous and exhausting journeys to acquaint himself at first hand with conditions abroad and devoted much intensive study to ways and means of providing effective help.

He had a special love and understanding of the peoples of India and a personal friendship with many of the great political and scientific leaders who, in the midst of difficulties arising from partition, were endeavouring to raise standards of living and re-shape the economy of their country. His department at the Imperial College of Science had long been the training ground for a succession of brilliant young Indian scientists and technologists, a number of whom were now called upon to assume the responsibility for a massive programme of expansion in the fields of education and commerce; and he received many pressing invitations from them to visit India and to advise upon some of its more urgent problems.

His first visit took place in 1948 when he was appointed chairman of a committee to review the working and development of the Indian Institute of Science, Bangalore; his fellow members on the committee were Sir Ben Lockspeiser and Dr J. N. Mukherjee. Their report was an eminently statesmanlike and practical document outlining a policy of development attuned to the prevailing conditions. It pointed out that an overwhelming proportion of the population was actively engaged in agriculture or was working in industries dependent on cultivated produce; at least 75 per cent. were organized in village communities. Apart from the probability or desirability of a fundamental change in this economy, the principal factor in raising the standard of living in the near future would be an increase in the productivity per person within the village communities and a balanced development of industry and agriculture. This would be achieved by assessment and wise utilization of natural resources, extension in irrigation, improvements of the soil and methods of cultivation, the provision of power, and the improvement of health standards and facilities of education. Too rapid a growth of population could wreck all attempts to improve standards.

The report then proceeded to outline proposals for building up scientific and technical manpower by a progressive expansion of educational facilities and by the setting up of uniform and appropriate standards of education.

The recommendations were very much in line with the views held by the more enlightened members of the Council of Ministers. They had always given full support to the Council of Scientific Research of which the Prime Minister was Chairman and Sir Shanti Swarup Bhatnagar, F.R.S., the Director. Egerton had long known and admired Bhatnagar and he now had an opportunity of discussing with him the plans for creating a number of national laboratories in which scientific research and development in support of Indian industry, agriculture and medicine could be conducted under Government auspices. He was invited to Delhi to stay with Pundit Nehru who was quick to recognize a kindred spirit, uniting his own high ideals with an absolute integrity. They became friends and on subsequent occasions when the Prime Minister visited this country he always set aside a few hours in which to renew their friendship.

In the intervals of work Egerton took every opportunity of acquainting himself with the social and economic conditions of the country. Whilst in Madras he and Lady Egerton were guests of the Governor General, Sir Archibald Nye, and later, at the invitation of the Government, they spent a week in Kashmir. At Srinagar on the banks of that lovely lake which brings back nostalgic memories to every Anglo-Indian, he was able to test his skill, with considerable success, against a peculiarly sophisticated breed of trout.

In 1954 he was again invited to India by Mr Nehru to inspect and report to the Government on the work of the sixteen national institutes and laboratories which had been established since his first visit. Associated with him were Professors Dupouy, S. N. Bose and D. A. Mudeliar, Shri Kaslierbhai Lalbhai, Sir Shri Ram and Sir Shanti Bhatnagar. This was a considerable task involving exhausting journeys by air, innumerable and detailed discussions of research programmes, laboratory organization, personnel problems and finance. Work began early in the morning and was often preceded by a ceremonial breakfast at which that dynamic personality, Bhatnagar, acted as host and 'catalyst'; the day was passed in inspections and personal discussions with the directors and staff of the institutes, and the evening was given up to writing reports.

Lady Egerton, who accompanied the party, was soon absorbed by the women's community and had in some respects a more strenuous time than her scientific companions. Political independence which had so stimulated the male population was not without its influence on their womenfolk. They were demanding greater freedom, higher standards of education and a larger share in the responsibility for building the new India. Lady Egerton's understanding of their aspirations, her unaffected interest in their complex problems and her experience of women's organizations in this country were of great value in their discussions and gatherings. The intensive work of the scientific delegates was now leavened by an impingement of social functions at which the broader aspects of Indian development and culture were freely debated before a more liberal audience. The combination of interests was most successful and might well serve as a pattern for future visits of this kind.

Having completed their labours the Egertons spent a few weeks with Sir Shanti Bhatnagar at his farm near Delhi. They were also able to re-visit Kashmir and recuperate for a while amongst its peaceful hills and lakes.

Egerton retired from his chair in 1952 but was given a private laboratory in his old department in which he continued his researches on combustion and maintained contact with his old colleagues.

The importance of his work was recognized by many awards and distinctions. He was elected Fellow of University College, London, of the Imperial College of Science and of the City and Guilds College, and he was given Honorary Degrees by the Universities of Birmingham, Cairo, Nancy and Helsinki. The Institution of Mechanical Engineers awarded him the George Stephenson Research Prize, the Herbert Akroyd Stuart Prize and elected him the Thomas Hawkesley Lecturer for 1940. The Institution of Civil Engineers awarded him the Telford Premium in 1942. He received the Rumford Medal of the Royal Society in 1946.

The British Coal Utilization Research Association appointed him its first Coal Science Lecturer in 1952. He was awarded the Hinchley Memorial Medal of the Institution of Chemical Engineers in 1954, the Melchett Medal of the Institute of Fuel in 1956 and the Egerton Medal of the Combustion Institute in 1958.

COMBUSTION RESEARCHES

Experiments on engine 'knock'

Egerton's interest in combustion dated from the inter-war years when endeavours to improve the performance and efficiency of the internal combustion engine had drawn attention to the need for a better understanding of the mechanism of oxidation processes. The early theories of ignition, flame propagation and detonation based upon chemical analyses of the products of combustion and crude measurements of flame temperatures and reaction velocities, were unable to provide a satisfactory explanation of the complex happenings in engine cylinders; in particular the incidence of knock and the action of inhibitors (anti-knocks) presented many puzzling features.

It was at first assumed that knock could be identified with detonation and that the effect of anti-knocks might be to delay combustion and hence to retard the setting up of a detonation wave. At the suggestion of H. T. Tizard, Egerton set out to obtain experimental evidence in support of this view. His first approach was to measure the velocities of detonation in tubes of acetylene- and pentane-oxygen mixtures at initial pressures up to 7 atmospheres and temperatures up to 230 °C. He found that for a given mixture increase of pressure diminished the distance from the source of ignition at which detonation occurred up to a certain limiting pressure after which it remained unchanged; contrary to expectations the addition of typical anti-knock agents such as lead tetraethyl and nickel carboxyl had no influence on the onset of detonation (A. Egerton and S. F. Gates 1927). These observations led him to examine the effect of anti-knocks on the self-ignition temperatures of petrol and pentane in air and he confirmed that, in most cases, the temperatures were significantly raised. By an ingenious experimental device he was able to show that it is the metallic part of an organo-metallic anti-knock which is mainly effective, that the metal atoms should be capable of forming two oxides and that they should be in an incipient state of oxidation. With regard to the causes of knock he concluded that a chain reaction was involved in which case anti-knocks would function by breaking the chains.

It now became necessary to trace the sequence of events occurring in an actual engine cylinder during a complete working cycle. To this end he employed a Delco engine fitted with a special valve arrangement which permitted samples to be taken at pre-determined stages of the compression and expansion strokes. A knowledge of changes in composition of the samples then enabled the progress of oxidation to be followed. The analyses of the gas mixtures proved to be a matter of extreme difficulty and involved working out methods for distinguishing between, and estimating quantitatively, inorganic and organic peroxides, aldehydes and acids present in small quantities in a mixture of oxides of carbon, nitrogen and steam. The results were found to be consistent with the view that knock is due to a special type of combustion occurring in a small portion of the charge in the cylinder which has undergone pre-sensitization, with the subsequent development of a branch chain reaction; the observed changes in concentration of peroxides and aldehydes during a cycle suggested that knock is intimately connected with the presence of peroxidic substances formed in the last part of the charge to burn. Such peroxides could be formed in the primary oxidation of the hydrocarbon or from aldehydes (A. Egerton, F. Ll. Smith and A. R. Ubbelohde 1935).

Concurrently with this work he studied the slow combustion of pentane and of a range of hydrocarbons and derivatives which might be formed in the early stages of its oxidation, viz. hexane, amylene, valeraldehyde, *iso*-pentane and *iso*-amyl alcohol. The action of lead tetraethyl as inhibitor of the oxidation was also noted (L. M. Pidgeon and A. Egerton 1932).

Egerton was now convinced of the important role played by peroxides in hydrocarbon combustion and thought it desirable to obtain more information on their ignition characteristics and modes of decomposition. He chose as typical examples diethyl and dipropyl peroxides, and ethyl hydrogen- and propyl hydrogen peroxides. The decomposition of diethyl peroxide in the temperature range 130 to 190 °C was found to be a homogeneous unimolecular reaction, becoming explosive at a critical pressure depending upon the temperature. In the slow combustion of pentane the addition of small quantities of the peroxide had the effect of almost suppressing the induction period. Dipropyl peroxide behaved in a similar manner. Ethyl- and propyl hydrogen peroxides, on the other hand, decomposed heterogeneously, behaviour which pointed to a connexion between these compounds and the initiation of slow combustion (E. J. Harris and A. Egerton 1937, 1938).

Limits of flame propagation

The advent of the turbo-jet engine in which large quantities of fuel have to be burnt completely and rapidly in as small a space as possible, led Egerton to consider the possibility of employing promoters or inhibitors to change the limits of inflammability of the fuel.

An investigation into the propagation of flame in limit mixtures revealed that the limits are mainly governed by the temperature of the flame and not by pre-flame reactions. The temperature must be such as to provide the necessary concentration of active radicals in the boundary region to inflame the entering gases. A definite correlation was found to exist between the heat of combustion and the composition of the limit mixture. The presence of promoters in the gas ahead of the flame had little specific influence on the propagation limits (A. Egerton and J. Powling 1948).

'Flat' flames

In the theoretical treatment of heat and mass transfer processes in flames it is customary to assume the existence of a stationary plane flame front and it occurred to Egerton that if such a flame could be formed and stabilized at a suitable burner accurate measurements could be made of flame velocities in slow burning mixtures and of the composition of limit mixtures. After much trial and error a successful flat flame burner was developed and employed in the measurement of the burning velocities of methane, ethane, propane, butane, ethylene, carbon monoxide and cyanogen in the range 4 to 8 cm./s. The results gave a straight line relationship between composition and burning velocity in all cases save that of carbon monoxide which was found to be sensitive to moisture content. The mixture law was found to hold for binary mixtures, those containing carbon monoxide being again an exception.

The flat flame burner was subsequently used for measurements of the effect of pressure on flame speeds and for the study of cool flame phenomena. It has also proved of great value in examining the structure and optical properties of flames (A. Egerton, J. Powling, S. Thabet and G. Badami 1952, 1954).

The oxidation of methane

Mention has already been made of Egerton's work on the use of liquid methane as a fuel, and this was only one instance of his interest in a gas of great potential industrial importance. When he was invited to give the Melchett Lecture for 1956 he chose for his subject 'Methane and its combustion' and gave an account of researches on its slow oxidation which had occupied him for many years. By establishing conditions which ensured reproducible results he had succeeded in proving that formaldehyde is formed from the start of the reaction, even in the so-called induction period. Similarly hydrogen peroxide was shown to be formed, though rather less rapidly in the early stages. No other intermediates such as methanol or methyl hydroperoxide were detected. The maximum rate of oxidation corresponded with a maximum in the concentration of hydrogen peroxide. This observation together with others on the effect of different surfaces in the combustion led him to conclude that when methane reacts slowly with oxygen there is a slow developing chain reaction providing radicals (CH_3, OH, HO_2) and the radicals are destroyed at the walls. But there is also a faster reaction in which formaldehyde is oxidized, providing HO_2 radicals which account for hydrogen peroxide production, and these can also account for ignition at high temperatures, just as they do in the hydrogen/oxygen reaction (A. Egerton, G. J. Minkoff and K. C. Salooja 1956).

These results and a suggestion by Mr Campbell Secord led him to consider whether very weak mixtures such as those found in ventilating air from mines could be burnt rapidly enough at high temperatures to provide recoverable energy. Experiments showed that methane-air mixtures containing from $\frac{1}{2}$ to 1 per cent. of the hydrocarbon burn readily at 950 to 1000 °C, the oxidation occurring in two well-defined stages. The desirability of obtaining kinetic data relating to combustion at temperatures of the order of those found in flames became apparent and Egerton was engaged in this work at the time of his death.

Space does not permit an account being given of the many other aspects of combustion to which Egerton made contributions but these are referred to in a number of reviews on the progress of combustion science which he published from time to time and which are listed in the bibliography.

(Acknowledgements are due to Mr Peter Moore for assistance in the preparation of this summary.)

Egerton's time continued to be very fully occupied during his retirement with the affairs of the Salters' Institute and of the Combustion Institute, of which he was Honorary President. He was Honorary Editor of *Fuel* and of *Combustion and flame*.

In 1956 he became adviser to the Tobacco Manufacturers' Standing Committee and supervised researches on their behalf.

He had long been connected with the work of the British Council and was for eleven years Chairman of its Scientific Advisory Committee and member of its Executive Committee. In 1957 he undertook on its behalf a tour of the Middle East visiting and lecturing at Beirut, Baghdad and Teheran.

From 1954-58 he served on the Anglo-Belgian Mixed Commission set up under the terms of the Cultural Convention between Great Britain and

Belgium, and in 1959 he was appointed to the corresponding Anglo-French Mixed Commission.

Egerton was now upwards of 70 years of age and although his health continued to be good there is no doubt but that the many tasks he undertook during his retirement imposed a strain on his physique. He seemed nevertheless to be untiring and to make no concessions to age; whatever the nature of his evening engagements they were invariably followed by several hours at his desk. In the intervals of work he enjoyed long and strenuous walks and, to the last, his pace over hilly country was such as to fatigue many younger men.

He usually spent several months of the year in France, a country for which he had a great affection. He had inherited from his mother a charming residence, Mas del Soleou, situated amongst the olive groves of Mouans-Sartoux in the Alpes-Maritimes. Here he delighted to work in the garden, to make his own wine, to entertain his friends and to ramble with them over the hills; and here it was he passed his last days in surroundings he loved and in the company of one who had been his constant companion for upwards of 47 years. He died peacefully on 7 September 1959.

The records of the Royal Society contain a sermon preached 268 years ago by Burnet, Bishop of Salisbury, at the funeral of one of its most illustrious Fellows, Robert Boyle. He chose for his text Eccles. ii, 26: 'For God giveth to a man that is good in His sight, wisdom, and knowledge and joy'; he spoke with much feeling of the unselfish life and great works of his old friend and ended: 'But his peculiar and favourite study, was Chymistry, in which he engaged with none of those ravenous and ambitious Delights, that draw many unto them. His Design was only to find out Nature, to see into what Principles things might be rendered, and of what they were compounded.'

To those who knew Jack Egerton, who shared in his labours and admired his achievements no text and no words could better serve to enshrine his memory.

D. M. Newitt

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H. Heilbronn

IAN MORRIS HEILBRON

1886-1959

IAN MORRIS HEILBRON, younger son of David Heilbron, was born on 6 November 1886 in Glasgow. He received his early education at the High School, Glasgow, where he became fired by an enthusiasm for chemistry. In later years he often recalled the disfavour with which his father, who was prominent in both the commercial and social life of Glasgow, viewed his determination to take up chemistry as a career, for, at the time when science still seemed to have little relation to industry, the choice appeared to offer only a limited academic career. Fortunately for chemistry and indeed eventually for all science, the young Heilbron was allowed to follow his own bent and entered the Royal Technical College, Glasgow, where he quickly came under the enduring influence of G. G. Henderson, F.R.S., an influence which Heilbron fervently acknowledged to the end of his days. It was at Henderson's insistence that he took up a Carnegie Fellowship at the University of Leipzig where he studied under Hantzsch from 1907 to 1909 and took his Ph.D. degree. Having come from an unusually cultured environment in Glasgow, he particularly enjoyed the musical life in Leipzig. At this period he began a life-long friendship with R. Robison, F.R.S., later to achieve distinction in the biochemical field, but in later years he rarely referred to his work in Germany although it clearly had a pronounced influence in impressing on him the immense assistance which the organic chemist could derive from the application of physical methods. There is little doubt that this early experience led Heilbron directly to pioneer in due course the development in particular of spectroscopy, high vacuum distillation and chromatography in this country.

On his return, Heilbron, again on the 'advice' of G. G. Henderson (see the first Henderson Memorial Lecture, *Roy. Inst. Chem.* 1947) became Lecturer at the Royal Technical College, Glasgow, until the outbreak of World War I. He had taken a Commission as Lieutenant in the R.A.S.C. in 1910, was posted overseas in the 52nd Division in 1915, and in 1917-19 served brilliantly, ultimately with the rank of Lieut.-Colonel, as Assistant Director of Supplies at G.H.Q. Salonika. He was three times mentioned in despatches, was awarded the Médaille d'Honneur of the Greek Order of the Redeemer, and at home was honoured with the award of the D.S.O.

After the war he worked for a short time in industry with the British Dyestuffs Corporation with which, in the later form of I.C.I. Ltd he maintained a close contact as Consultant until 1949. However, academic life continued to attract him and from 1919-1920 he was Professor of Organic Chemistry at the Royal Technical College, Glasgow, from 1920-1933 Heath Harrison Professor of Organic Chemistry in Liverpool, 1933-1938 Professor of Organic Chemistry (1933-1935), later Sir Samuel Hall Professor of Chemistry in Manchester, and from 1938-1949 Professor of Organic Chemistry at the Imperial College of Science and Technology, University of London. In each of these centres Heilbron came at an opportune moment to reorganize the teaching of organic chemistry and mostly this necessitated extensive physical refashioning of the whole department. To have carried out these tasks with the energy, brilliance and foresight which Heilbron so successfully brought to them would alone have indebted the scientific world to him. There are, however, still more substantial memorials to his work. One is surely reflected in his many publications, to some of which reference is made below. Another and possibly the most lasting and productive outcome of his efforts is associated with the capacity which he had to gather round him from many parts of the world young chemists of promise. Many of these proved apt pupils in the study of his personal approach to the organization of scientific work and have themselves since become leading figures in either academic or industrial life at home or abroad. It may be mentioned in passing that Heilbron, who was a member of the community of Liberal Jews, sometimes remarked that he found difficulty in accepting the conventional religious view of 'life hereafter', but that he was happy to feel that his scientific life would continue after his physical life had ended, through his many former colleagues.

He retired from academic life in 1949 to become in effect the first Director of the Brewing Industry Research Foundation where he remained until 1958. At the outset the Foundation was still but a general conception and it was largely Heilbron's indefatigable energy, wide experience and lively imagination touching upon every detail of the new organization which eventually gave the establishment a form and cohesion such as few others could have achieved.

Soon after coming to the Imperial College, Heilbron's services were being sought in connexion with the war effort. So he acted as one of the Scientific Advisers to the Ministry of Supply from 1939 to 1942 and Scientific Adviser to the Ministry of Production from 1942 to 1945. During this period he played a leading part in the introduction of D.D.T. which had a significant effect on the conduct of the war in the Far East, in Africa and in the later phase in the Mediterranean area. He had improvised sleeping accommodation at his laboratory in London and was thus with exceptional energy able to maintain also the direction of the University department. Indeed, during the war and shortly afterwards, publications under his name appeared with increased frequency on a diversity of topics. For his war service he was knighted in 1946 and received the American Medal of Freedom in 1947, but his public service continued, for at different times after the war he was Chairman of the Colonial Insecticides Committee, Member and also Chairman of the Advisory Council on Scientific and Industrial Research, and Member and Chairman of the Advisory Council of the Royal Military

College of Science. Moreover he was still, with so many other commitments, able to give valued advice over a wide field of industry and also for some years after the war was a leading figure in reorganizing the International Union of Pure and Applied Chemistry.

His great distinction led to many academic honours. He had been elected a Fellow of the Royal Society in 1931, received honorary degrees from Glasgow and Edinburgh, served as President of the Chemical Society (1948-1950), and was an Honorary Member of the French Chemical Society and a Foreign Member of the Royal Netherlands Academy of Sciences. In addition he had been honoured by many learned societies as by the award of Longstaff, Hugo Müller and Pedler Medals and Lectureships (Chemical Society, 1939, 1940 and 1947 respectively), Davy and Royal Medals (Royal Society, 1943 and 1951), Priestley Medal (American Chemical Society, 1945) and Horace Brown Medal (Institute of Brewing, 1959).

Apart from some 300 publications which dealt with his original contributions to knowledge, mention should be made of his part as Chairman of the Editorial Board in producing the latest edition of Thorpe's *Dictionary of Applied Chemistry* and in particular of his part, with H. M. Bunbury in compiling his *Dictionary of Organic Compounds* and piloting its several volumes through a number of editions as Editor-in-Chief.

Perhaps the most marked characteristic of all Heilbron's work was the meticulous attention which he paid to detail. None of his papers, lectures or even ordinary correspondence was ever merely 'written'. All was written, then recast and rewritten, often several times over. Frequently a whole morning might be spent on one paragraph. Often the sole outward result, despite numerous passing references to dictionaries and scientific journals, was the insertion of a single adjective or some similar minor alteration. The outcome, however, usually seemed worth while for he would say 'I always knew there was something wrong with it' and there was an obvious satisfaction in having perfected what to many would have earlier seemed beyond criticism. In the same way each of his spoken contributions, irrespective of whether it was for an international audience or perhaps for a much less formal occasion such as a meeting of a students' society would be prepared to the extent that literally each word was selected to express precisely and logically what he had in mind. Each illustration was purposefully designed so that all the essential data were included while unnecessary features were excluded. The result was invariably a performance which held his audience by its seeming spontaneity and at the same time by its obvious directness and unhesitating logic. Indeed, it was an important part of his artistry in this respect that first he always formulated a distinctive narrative and then told it in his own distinctive way. As the years went by so that he drew upon an almost unsurpassed experience in University teaching, Government service and as well in industrial research, his reminiscences became more cogent, for right up to the time of his death he was constantly looking as much to the future as to the past, always in touch with reality. There must be many colleagues on the numerous committees on which he served who will recall the seemingly psychic clarity with which he came to view every issue. A topic might have been discussed at great length apparently without a satisfactory course of action having emerged, when Heilbron would often put forward a proposal of almost dramatic simplicity which filled his colleagues with admiration and at the same time baffled them for having themselves failed to arrive at so obvious a solution. Certainly in later years he was well aware of his ability to anticipate in general terms with seemingly uncanny accuracy the reactions of those with whom he had to deal. Indeed he himself ascribed it to a well-developed but innate Jewish acumen though to most it was but one facet of his extraordinary mental ability.

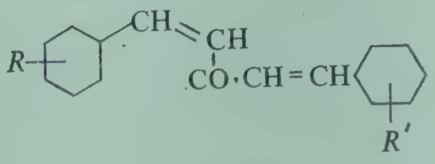
All this, however, had to do with his public work. In private life he was one of the most delightful companions imaginable. Everything around him, for instance, his old English furniture, his collection of modern paintings and his fine antique porcelain, betokened the sensitive nature and those who were privileged to know him more closely were constantly being surprised by his wide appreciation of music, literature, other forms of art and indeed of human nature in general.

Between the extremes of public and home life there was the professor of chemistry known to some thousands of former students, several hundred of whom had carried out research under his direction. To them he, despite his small stature, could be frightening for he was always impatient of delay or of a lack of direct and constructive thought. The research student who sought to play for time by making such remarks as 'I want to characterize it as a semicarbazone' soon learnt to exercise nicer distinction when silenced by the reply 'I'm not stopping you'. The man who, to offset criticism of his report, might refer to it as a 'rough draft', would be reduced to a state of acute anxiety by withering opinions of what was to be expected from 'educated university graduates'. Heilbron's nature as a professor was, however, really one of scrupulous fairness and great kindness for he was always ready to help the student who responded to what was offered him. It would be difficult to imagine a professor who showed more concern for the welfare of his students, a concern which mostly continued in an unobtrusive but exceptionally generous manner long after his protégés had been launched on their own careers.

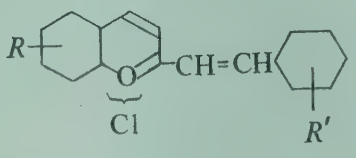
Heilbron's earliest research work (with the late G. G. Henderson) emphasizes today the immense span of his experience. It was concerned with various reactions of pinene, bornylene and camphene, more particularly in the latter cases with the structural relationship between the two. The formula of camphene accepted today had some time earlier been proposed by Wagner but the rearrangement which now bears his name was not at the time understood and Henderson and Heilbron still felt on the basis of their painstaking work that the two hydrocarbons were identical in ring structure as regards the greater part of their respective molecules.

On his return to Glasgow from Leipzig, Heilbron in association with F. J. Wilson took up what was at the time the novel study of semicarbazones, especially with regard to the phenomenon by which various carbonyl compounds such as phenylstyryl ketone and *m*-nitrobenzylidene-deoxybenzoin gave rise to two or more forms of semicarbazone. It is remarkable that from the first Heilbron in a pioneering manner placed great value on evidence derived from absorption spectra and was soon distinguishing with confidence between cases of stereoisomerism as in the case of phenylstyryl ketone, of structural isomerism as with mesityloxide and of tautomerism as in the 'semicarbazones' of various quinones (with J. A. R. Henderson). At the same time the ability of semicarbazones to combine loosely with acids such as anhydrous hydrogen chloride was discovered (with F. J. Wilson, M. M. J. Sutherland and J. A. R. Henderson) but these investigations lapsed soon after the outbreak of World War I.

On returning to academic life in Liverpool in 1921, Heilbron appeared at first to be still mainly interested in structural questions in that a series of papers appeared (with J. S. Buck) dealing essentially with chromotropism and varying chemical reactivity among doubly conjugated unsaturated ketones and particularly among various distyrylketones (I). The high degree of reactivity among such compounds was believed to be due to internal complex formation. Interest indeed shifted to the benzopyrilium salts (II) of distyryl ketones in which ring formation could be given definite structural expression. In the case of the *p*-hydroxy compounds (II, R′ = *p*—OH) the colour changes which followed on treating the chlorides with alkali were regarded as being due to the formation of *p*-quinonoid structures (III). Analogous quinonoid anhydrobase structures



(I)



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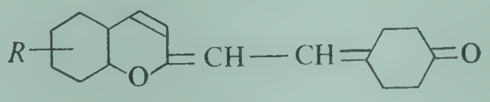


(III)

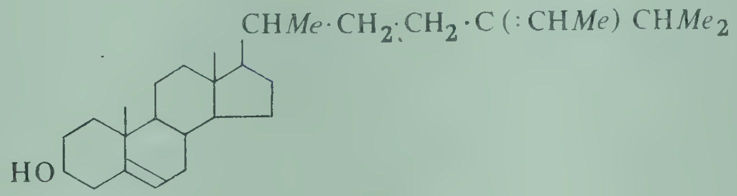
were suggested on this basis for the anthocyanidins. It is of interest to note that it was appreciated that this suggestion made it necessary to revise the structure proposed by Willstätter for malvidin which failed to make provision for the necessary *p*-hydroxyl group, a necessity which was later borne out.

During this period when he was clearly feeling his way towards still more sustained research, Heilbron carried out a certain amount of tentative work in other fields still concerned with 'classical' organic chemistry. This included studies on such diverse topics as the chemistry of meso-thioanthracene compounds (with J. S. Heaton, W. H. Cooke and G. H. Walker) and the reactivity of methyl groups as for instance in the 2-position of the chromone (with H. Barnes and R. A. Morton) and of the 4-quinazolone rings (with F. N. Kitchen, E. B. Parkes and G. D. Sutton). Heilbron was always ready to turn his attention to a new aspect of organic chemistry and such miscellaneous studies continued over the next thirty years, but he was destined to make a still greater impact on the study of natural materials.

Shortly after assuming the professorship in Liverpool, Heilbron was collaborating with E. C. C. Baly and their colleagues on the apparent photocatalysis of the conversion of carbon dioxide into formaldehyde and thence into carbohydrates and other substances in presence of various coloured materials including chlorophyll. The findings were extended to a consideration of the origin of various nitrogenous compounds in plants. Although Baly believed passionately that the formaldehyde in question was formed by photosynthesis, Heilbron repeatedly related in later years that the results were vitiated by uptake of organic material from the rubber tubing used in the apparatus and he turned to a different and seemingly even more difficult field in the chemistry of fish liver oils, in the first instance of squalene (spinacene). Largely by his unwavering eagerness to draw upon all possible aid from physical measurements, Heilbron was soon convinced of the identity of squalene with spinacene and at an early date (with E. D. Kamm and W. M. Owens) had correctly assigned the empirical formula $C_{30}H_{56}$, had put forward the structure which in all essentials is accepted today and drawn attention to the probable relationship between this compound and various sterols. From this point, his interest became mainly centred on the chemistry of natural compounds, at first on the antirachitic activity developed in 'ordinary' cholesterol upon irradiation, an activity soon shown (with R. A. Morton and E. D. Kamm) to be due to the production of vitamin D from ergosterol. Soon structural changes undergone by cholesterol (with W. A. Sexton), the occurrence of zymosterol in yeast and of monoglyceryl ethers (selachyl and batyl alcohols) in fish oils (with W. M. Owens) and other topics were being investigated, but the characterization of ergosterol, its occurrence and spectroscopic identification in a wide variety of fish liver oils (with W. A. Sexton, E. D. Kamm, R. A. Morton and later J. C. E. Simpson) were a main interest and, within the context of the structures accepted at the time, revealed a number of new interrelationships as well as establishing correctly the empirical formula $C_{28}H_{44}O$. Simultaneously, Heilbron took up related work on the nature of the substituted naphthalenes and other products obtained on dehydrogenating squalene (with J. Harvey and D. G. Wilkinson) and broadened the interest of the whole line of inquiry by showing (with R. A. Morton) that the content of vitamin A in fish liver oils could also be assessed spectroscopically, observations which still provide the basis of the most reliable method of assaying the vitamin. From this point the chemistry of the sterols generally and also of vitamin A permeated most of Heilbron's work for several years.

A spate of publications from his pen did much to establish the general nature of vitamin A, the extent of its occurrence in various fats and oils, the preparation and characterization of its derivatives and in due course, the detailed structure in which, however, he was narrowly anticipated by Karrer and his co-workers. There can be no doubt on the other hand of the great permanent value of the painstaking and essential if sometimes unspectacular work which Heilbron carried out, largely in association with R. A. Morton, F.R.S., and the late J. C. Drummond, F.R.S.

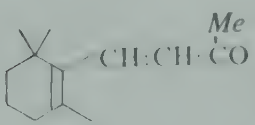
The flood of diverse communications on such topics as vitamin D-, vitamin A-, carotene- and xanthophyll-contents of butter, the occurrence of vitamin A in small quantities of egg yolk (with A. E. Gillam) which went on for some years from about 1932-1937, was broadened still further by a growing interest in carotenoids as well as sterols and bile acids themselves. Shortly after the revolutionary proposals regarding the general structure of the sterols by Rosenheim and King and by Wieland and Dane in 1932, Heilbron (with J. C. E. Simpson and F. S. Spring) had correctly deduced in all important essentials the structure of ergosterol. With this background, he embarked on a detailed study of the chemistry of the algae and (with R. F. Phipers and H. W. Wright) discovered and worked out the structure of fucosterol (IV), the characteristic sterol of the seaweeds,



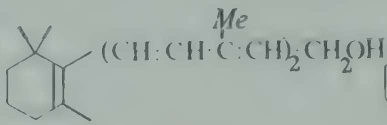
(IV)

Turning to the carotenoids, various remarkable facts emerged. For instance, it became apparent that fucoxanthin, the characteristic pigment of the brown algae and in particular of *Fucus vesiculosus*, could be isolated only from fresh material, the drying of which was associated with the conversion of the pigment into zeaxanthin, the typical colouring matter of maize (with R. F. Phipers). Wider study revealed that each group of algae were associated with characteristic carotenoid pigments in which a gradual transition from the normal composition of land plants in this connexion could be discerned (with E. G. Parry and R. F. Phipers). This work was delightfully summarized in the Hugo Müller lecture in 1941 in which it became clear that the culminating point was the finding that in the dioecious Fucales such as *Fucus serratus* the female gametes are pigmented largely by fucoxanthin (apart from chlorophyll) whereas the male gametes owe their colour almost entirely to β-carotene. Heilbron (with P. W. Carter, L. C. Cross and E. R. H. Jones) clearly appreciated the possibility that this differing pigmentation may be associated with the mobility of the male gametes and perhaps with their attraction to the egg, but the full significance of these findings still remains to be elucidated.

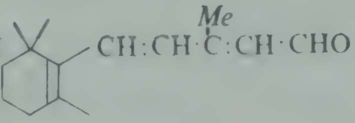
Naturally the possibility of synthesizing vitamin A and even polyene pigments themselves proved an early attraction. Much of the laborious work was at first concerned with attempts to use β-ionone (V) to develop methods of building up the necessary extended side chain in the vitamin (VI) itself and, while obstacles were being overcome, to put accumulating experience to good use by obtaining analogous compounds. For instance, considerable effort was first devoted to synthesizing the C_{15}-aldehyde (VII), *e.g.* by the condensation of β-ionone with acetylene followed by rearrangement of an intermediate acetylenic carbinol. A satisfactory synthesis of the vitamin, however, proved elusive by routes of this type owing to the anomalous behaviour of β-ionone and many of its derivatives. In consequence, Heilbron's attention became focused on the potentialities of the C_{14}-aldehyde (VIII) or its equivalent (IX). He soon perceived that an aldehyde which had been claimed to have structure (VIII) in 1937 was actually derived from α-ionone and therefore useless. There is, however, no doubt that his painstaking work (with A. W. Johnson, E. R. H. Jones and A. Spinks) in synthesizing the required aldehyde from β-ionone provided the basis on which the synthesis of the vitamin itself was ultimately based. Heilbron envisaged the synthesis as involving the incorporation of (IX) with acetylene and a ketobutanol derivative to give the required carbon skeleton:



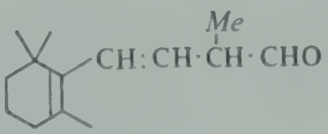
(V)



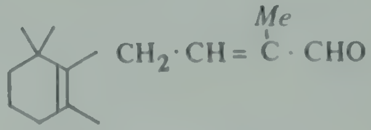
(VI)



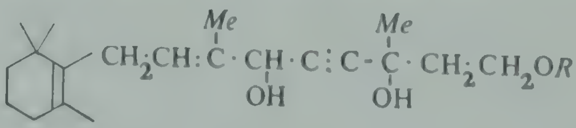
(VII)



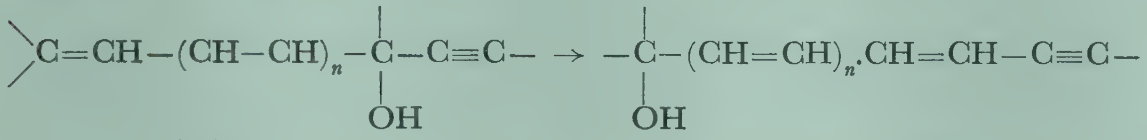
(VIII)



(IX)



This scheme permitted many variations but unfortunately the conditions during war-time and particularly the lack of β-ionone prevented its full exploration. Its correctness was nevertheless completely established by the synthesis carried out by Isler *et al.* (*Helv. Chim. Acta*, 1947, **30**, 1911). Meanwhile Heilbron and his school were taking full advantage of the opportunity to study the condensation of $\alpha\beta$-unsaturated carbonyl compounds with acetylenic compounds of various types. Here perhaps one of the most fruitful results has been to establish the generality of the anionotropic rearrangement whereby acetylenic carbinols typified by (X) undergo a change such that completely conjugated compounds of type (XI) are formed (with E. R. H. Jones and co-workers).



$$\rangle C{=}CH{-}(CH{-}CH)_n{-}\underset{OH}{\overset{|}{C}}{-}C{\equiv}C{-} \rightarrow {-}\underset{OH}{\overset{|}{C}}{-}(CH{=}CH)_n.CH{=}CH{-}C{\equiv}C{-}$$

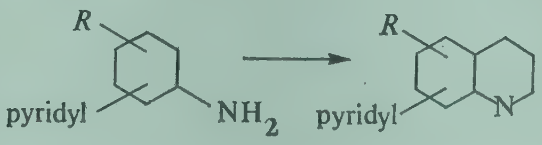
(X) (XI)

These basic reactions together with numerous variants and elaborations led to many quite new types of compound and indeed the diverse possibilities so opened up will undoubtedly continue to form the subject of research efforts in widely separated laboratories for many years to come.

While this voluminous work was gathering momentum, other sections of Heilbron's growing school of research were pursuing quite different but still remarkable lines of inquiry. He had become interested in studies on the Gomberg reaction carried out in his Department (W. S. M. Grieve and D. H. Hey, *J. Chem. Soc.* 1938, p. 108) whereby the interaction of diazonium compounds with various other benzenoid substances led to union of the aromatic rings (with D. H. Hey and R. Wilkinson):

$$Ar{\cdot}N_2OH + Ar'H \rightarrow Ar{\cdot}Ar' + N_2 + H_2O$$

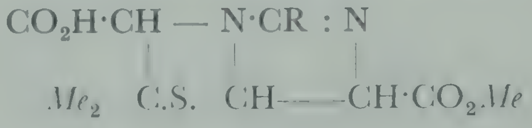
These studies led, among other results, to the synthesis of many substituted diphenyls, phenylnaphthalenes, terphenyls and even to a range of phenylpyridines and pyridyldiphenols (with J. W. Haworth and D. H. Hey). Even this rich harvest failed, however, to satisfy Heilbron who turned the accumulated experience in this field to the synthesis, mostly via appropriate nitro- and amino-phenylpyridines, of a wide range of pyridylquinolines which acquired considerable interest because of their promise as antispasmodics in the therapeutic field (with H. Coates, A. H. Cook, D. H. Hey, A. Lambert and in part F. B. Lewis and others):



During World War II when all these far-reaching investigations were being pursued at a phenomenal rate, especially considering the demands of government service, Heilbron's tireless energy still made it possible for him to take up the question of the chemistry of penicillin. So, in association with the writer and numerous colleagues, methods of purifying the antibiotic were devised so as to provide concentrates suitable for structural studies. There followed, as part of the collaborative Anglo-American effort in this connexion, a succession of reports, mostly reflected in later contributions to *The chemistry of penicillin* (Princeton University Press, 1949). Among the more general topics mention may be made here of the recognition of the formation of a dihydropenicillin-I (dihydropenicillin-F or *n*-amylpenicillin) as distinct from the penicillins-I and -II (2-pentenyl- and phenylacetyl-penicillins or -F and -G respectively) then being studied notably in Oxford and the United States. This led at length to the view that dihydro-penillaldehyde had the structure (XII) as was speedily proved by synthesis, a finding which

$\mathrm{CH_3(CH_2)_4CONH{\cdot}CH_2CHO}$

(XII)

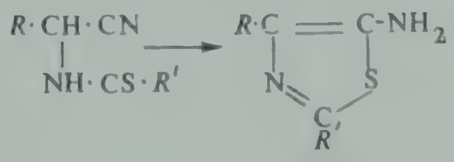


$$\begin{array}{lll} \mathrm{CO_2H{\cdot}CH} & \mathrm{-\ N{\cdot}CR} & \mathrm{:\ N} \\ \quad\ | & \quad | & \quad | \\ Me_2\ \mathrm{C.S.} & \mathrm{CH} & \mathrm{-\!\!-\ CH{\cdot}CO_2}Me \end{array}$$

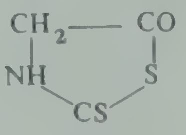
(XIII)

at once opened up the possibility of synthesizing *n*-amylpenicillin. As is now well known, the synthesis of the various penicillins continued to tax the combined efforts of many chemists. However, the early synthesis by Heilbron and his team of *n*-amylpenillamine and later, among many other key substances, of a DL-compound with the ring structure of the penillic acids (XIII) did much to stimulate efforts towards obtaining the antibiotics by synthesis. Heilbron's contribution to this general topic was by no means limited to synthetical approaches. It is noteworthy, for instance, that his group was the first to recognize the existence of *p*-hydroxybenzylpenicillin (penicillin-III or -X) and to establish the feasibility of obtaining it by deliberately bringing about its biosynthesis.

It will be evident that it was characteristic of Heilbron's approach to a problem that he was never content to direct his efforts solely to the prime object. Each project was seen as opening up wide fields of exploration often in directions which for a time seemed widely divergent from the major goal. So his work on the attempted synthesis of the penicillins soon led to other achievements including a new synthesis of methionine and similar amino-acids (with J. R. Catch, A. H. Cook and A. R. Graham) and, more significantly, to the synthesis of a variety of new classes of heterocyclic compounds such as (XIV) typified by the reaction (with A. H. Cook and A. L. Levy):

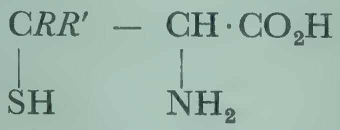


(XIV)



(XV)

Where R' was specially reactive as with ·OH, SH and $\mathrm{NH_2}$ the ring compounds were found to undergo a variety of reactions under mild conditions. For instance, when R=H and R'=SH, the cyclic aminocompound was readily converted into the thiazolone (XV) which in turn by a neat sequence of reactions was made to yield a variety of substituted cysteines (XVI).



$$\begin{array}{ccc} \mathrm{C}RR' & - & \mathrm{CH\cdot CO_2H} \\ | & & | \\ \mathrm{SH} & & \mathrm{NH_2} \end{array}$$

(XVI)

Furthermore, by the introduction of appropriate substituents, the 5-membered ring was made to form the basis of numerous new pyrimidines and pyrimidine analogues (*e.g.* with A. H. Cook, S. F. Macdonald and A. P. Mahadevan).

Heilbron had in 1924 married Elda Marguerite Davis, daughter of H. J. Davis of Liverpool, and had two sons both of whom survive him. Lady Heilbron had in every possible way shared his love of the arts and in addition, as was soon obvious to all who had the privilege of knowing her, possessed to a rare degree a kindly and sympathetic understanding of human nature. Her sudden passing at a comparatively early age in 1954 was understandably a great shock to Heilbron, one from which he possibly never fully recovered. His mental powers remained as keen as ever they were and his physical capacity was still quite exceptional, but the 'thoughts that breathe and words that burn' were perhaps coming with rather less vigour for some time before his sudden death on 14 September 1959.

In this necessarily brief account of Heilbron's life and work much has had to be omitted. Let it be hoped, however, that what has been written may convey some reflexion of a great man. For Ian Heilbron was more than a unique chemist. He came to belong to all science in the university, industry and government service both at home and abroad. Nor did he excel only in the politics of scientific advance for he became to an extent that few others have become, an elder statesman of science.

A. H. Cook

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F Joliot

JEAN FRÉDÉRIC JOLIOT

1900-1958

Jean Frédéric Joliot was born in Paris on 19 March 1900. He was the youngest of six children. His father, who was 57 when Frédéric was born, had fought in the 1870 war and afterwards in the Commune, as a result of which he had to flee to Belgium. When he returned after the amnesty, he became a business man and devoted himself to the education of his family. Frédéric spent 7 years at the Lycée Lakanal and then two at the Ecole Lavoisier, after which in 1919 he became a pupil at the Ecole de Physique et Chimie Industrielles de la Ville de Paris, where Paul Langevin was his teacher and was the first to recognize Joliot's abilities. From 1922 to 1924 Joliot worked in the steel industry in Luxembourg. Then after a year's military service he was appointed, on Langevin's recommendation, to the post of assistant to Marie Curie at l'Institut du Radium de Paris. Joliot's scientific career had begun in one of the greatest schools of experimental physics of the first half of the twentieth century. A year later he married Madame Curie's eldest daughter, Irène, who was born in 1897 and who was then engaged in research in her mother's laboratory. Thus started a scientific collaboration which ended only with Irène Joliot-Curie's death in 1956, two years before her husband's. During the first ten years of their married life, most of their scientific work was carried out together—26 joint papers were published during this period.

Joliot's first research concerned a new method of studying the electrolytic deposition of radio-elements (1).* The bottom of the vessel containing the electrolyte consisted of a very thin sheet of mica made conducting by a sputtered gold film. Polonium was electrolyzed out of solution on to this surface and the alpha rays emitted passed through the mica sheet into an ionization chamber below. Thus the rate of deposition of polonium could be followed quantitatively and continuously as a function of the electrode potential and the time.

This elegant experiment evidently led Joliot on to the study in detail of some of the physical properties of very thin metallic films (3). In particular the specific electrical resistance of sputtered platinum films was studied and found to be very large and to depend, to a considerable extent, on the conditions of sputtering. When a given film was heated, the resistance increased linearly and reversibly up to a certain temperature, after which it decreased irreversibly. On cooling again, a linear relation was again found.

* Numbers in parenthesis refer to the numbered bibliography at the end of the notice.

The irreversible changes observed were attributed in part to recrystallization effects and in part to the effect of heating on the occluded gases. As the thickness of the film was increased—the thickness was obtained by weighing with a micro-balance—the specific resistance gradually fell to that of the bulk metal. Later Joliot (13) tried out, or suggested several applications for such thin films; in particular for the construction of sensitive bolometers of very small heat capacity and also for thermo-junctions, for which purpose the advantages of the later much used lead sulphide was remarked on: transparent films of metal salts such as silver iodide were studied and the technique of making films of gold 100 Å thick without support was worked out. This latter method was utilized by G. P. Thomson in his pioneer experiments on the diffraction of electrons.

Joliot's second paper, in collaboration with Onoda (2), describes a very precise determination of the Bragg curve of polonium in hydrogen, and the stopping power of this gas relative to air.

In 1927 he published his first paper with his wife, Irène Curie. This was a new and precise determination of the number of ions produced when alpha particles from RaC′ and from polonium are completely absorbed in air. This number was an important constant, particularly at that time, since on it depended the estimation of the number of disintegrations a second from one gramme of radium. This was followed by a number of papers, some with Irène Curie and some alone. The main subjects were the properties of thin films, the electro-chemistry of the radioactive elements, and the nature of soft rays accompanying the emission of alpha rays from polonium. In addition Joliot (11) attempted to improve on Jacobsen's determination of the mean life of RaC′ as being about 10^{-6} second: this was about a hundred times larger than the value obtained from the extrapolation of the Geiger–Nuttall relation. Joliot found that the lifetime was less than 10^{-6} second. Later experiments by others have shown that the real value is $1{\cdot}45 \times 10^{-4}$ second. Joliot much later stated that he still did not know what went wrong with his measurements.

About 1930 Joliot began to use the Wilson cloud chamber for the study of radioactive disintegration processes, in particular for the study of the heavy recoil atom when an alpha particle is emitted. For this purpose he designed a novel type of chamber which contained little but water vapour at room temperature, so that the pressure at the end of the expansion was about 1 cm Hg, giving ranges of the recoil tracks some 76 times that corresponding to atmospheric pressure (44). Using actinium emanation in the cloud chamber (14) the recoil tracks of the heavy atoms produced when an alpha particle was emitted had a length of some 7 mm. It was thus possible to study in great detail the very complicated recoil tracks. In many cases two or more tracks diverged from the point of disintegration. Measurements of the angles suggested that the additional particles set in motion must have been heavy and were most likely atoms of actinium emanation and AcA, which must have been members of a compact group or cluster, before one of them disintegrated. Many branches were observed along the recoil tracks: these were attributed to collisions with the light atoms of the gas in the chamber. This study of heavy recoil tracks was to prove useful many years later for the study of fission.

In 1931 the two collaborators (15) described the preparation of a very strong source of polonium of the order of 200 millicuries, by a combination of electrolytic and volatilization methods. This considerable technical achievement was to prove of vital importance during the following year. At that time the source was the largest ever prepared. Strong sources of the natural radioactive elements were, at that time of course, the essential basis of the investigation of nuclear structure—they played the role then which the big accelerating machines play today. Very considerable experimental skill and a detailed knowledge of radio-chemistry were required for this task and here the long and fine tradition of Marie Curie's laboratory played an important role. Both Joliot and his wife were themselves skilled radiochemists and the former had taken the electro-chemistry of the radioelements as the subject of his thesis for his doctorate in the previous year (8).

This strong source of polonium was put to exciting use at the end of 1931 and the beginning of 1932. The Joliot–Curies were following up the discovery by Bothe and Becker in Germany in 1930 of the emission from boron and beryllium, when bombarded by alpha particles from polonium, of a radiation which was much more penetrating than any gamma rays from natural radioactive substances. Bothe and Becker attributed this radiation to gamma rays of several million volts energy emitted from the bombarded nuclei. Joliot and Irène Curie energetically followed up this work making use of their very strong polonium source, and very rapidly made a quite unexpected and very important discovery, published in the *Comptes Rendus* on 18 January 1932 (17). Using an ionization chamber with a thin window, they found that the radiation from beryllium, when bombarded by alpha rays, had the property of setting hydrogen nuclei in rapid motion. This was deduced from the following experiment. The radiation was passed through a filter of 1·5 cm of lead into an ionization chamber. When thin sheets of different substances were placed between the filter and the ionization chamber, no appreciable change in intensity was observed, except when substances containing hydrogen, such as paraffin, water or Cellophane were used, in which case a marked increase, in some cases a doubling of ionization was observed. When thin films of aluminium were placed between the paraffin absorber and the ionization chamber, it was found that the secondary and highly ionizing radiation was completely absorbed in 0·2 mm of aluminium. These simple and elegant experiments led to the inescapable conclusion that the penetrating radiation from the beryllium when bombarded by alpha particles was capable of setting hydrogen nuclei in rapid motion. This conclusion was soon confirmed directly by observing the tracks of the protons with a Wilson cloud chamber (19). The Joliot-Curies attempted to explain these results by supposing that the rays were photons of extremely high energy. They were quite aware of the difficulty of maintaining this view, since they calculated that photons of 50 million eV would be needed to produce, by the Compton mechanism, the high energy protons observed, and that the probability of such a process according to the Klein Nishina formula, was far too small. They recognized therefore that their interpretation required one to postulate a new mechanism of interaction of photons with matter.

The correct theoretical interpretation of these striking results fell to Chadwick at the Cavendish Laboratory, Cambridge. As soon as he read in the *Comptes Rendus* issued on 18 January 1932 of the Joliot-Curies' discovery of the projection of fast protons, he recognized that the rays responsible could not be energetic gamma rays, as the Joliot-Curies thought, but must be the long-thought-of and searched-for neutron.

It is not difficult to hazard the guess that if the Joliot-Curies had delayed publication of their experimental results for a few weeks they might themselves have been led to the correct explanation of their results. As an indication of the speed with which the researches in the two countries progressed one notes that between December 1931 and June 1932 the Joliot-Curies published seven notes in the *Comptes Rendus*, and that Chadwick's account of his experimental and interpretative work was published in *Nature* on 17 February, just four weeks after the Joliot-Curies' first publication.

In a survey of his researches written a few years ago Joliot pointed out that few discoveries can be traced to such separate distinct steps. First came Bothe and Becker's experimental discovery in 1930 in Germany that certain light elements emitted very penetrating radiation when bombarded by alpha particles. Then in January 1932 in France the Joliot-Curies showed that this radiation could set light nuclei in violent motion. Finally, just over a month later Chadwick in England confirmed the results and showed they implied the existence of the neutron.

The next important series of experiments carried out by the Joliot-Curies concerned positive electrons. In this case the initial discovery was by Carl Anderson in the U.S.A. in September 1932, who identified the track of a positive electron amongst the tracks of cosmic ray particles in a cloud chamber. In the early spring of 1933 Occhialini and Blackett confirmed the existence of these particles by the counter controlled cloud chamber method and related their production and disappearance to Dirac's theory of 'holes'. The Joliot-Curies energetically entered this field (25, 26, 27) and made important investigations into the formation of positive electrons by gamma rays from radioactive sources. Amongst other things they investigated the production of positrons in elements of increasing atomic number and showed that more positrons were produced but with the same maximum energy—this agreed with the predictions of the Dirac theory. They obtained the first recorded photograph of the production in the gas of an expansion chamber of a pair of positive and negative electrons. They also deduced the existence of a process of internal materialization in which an excited nucleus emits an electron pair instead of a gamma ray. Oppenheimer later gave the first theoretical treatment of this effect.

The annihilation radiation of positive electrons was studied by Joliot, employing Thibaud's method using the trochoidal path of electrons in the fringing field of a magnet and demonstrated that the photons were indeed nearly all of energy about mc^2, that is about 500 keV, in agreement with Dirac's theory.

From an analysis of the energy of the radiations in the transmutation of boron-10 the Joliot-Curies deduced that the neutron must be heavier than the proton and so unstable against decay into a proton and an electron (29), though they greatly over-estimated the energy of decay. Certain difficulties in understanding some of the observed transformations led them to envisage that a nucleus with an excess of protons might capture an atomic electron. The final proof of the existence of this process of electron capture was made by other workers a few years later.

The study of the emission of both neutrons and positive electrons from light nuclei bombarded by alpha particles led naturally to the greatest achievement by Frédéric Joliot and Irène Curie: this was the discovery of artificial radioactivity. In a short paper (38) published on 15 January 1934, they described how by simple measurements with a Geiger counter they had observed that, whereas the emission of neutrons ceased immediately the source was removed, the emission of positrons continued, decaying exponentially with the time as for a normal radioactive element. The elements aluminium, boron and magnesium gave decay periods of 3 min 15 s, 14 min and 2 min 30 s respectively. The initial intensities were relatively large: about 150 counts a minute were found for all three elements using a source of 60 millicuries of polonium. The elements H, Li, C, Be, N, O, F, Na, Ca, Ni, Ag gave no effect: the Joliot-Curies thought that the activity possibly existed but had too short a period to be observed with their experimental arrangement. That the rays were positrons was shown by the trochoidal method for aluminium and boron. When the energy of the alpha rays was decreased the intensity of the activity was decreased but the period of decay remained unchanged. These results suggested the existence of a new type of radio-activity with the emission of positrons. For aluminium the reaction seemed to be

$$\mathrm{Al}_{13}^{27}+\mathrm{He}_{2}^{4} \longrightarrow \mathrm{P}_{15}^{30}+\mathrm{n}_{0}^{1}$$

The new isotope of phosphorus formed would then decay with the emission of a positron thus

$$\mathrm{P}_{15}^{30} \longrightarrow \mathrm{Si}_{14}^{30}+e^{+}$$

Similar reactions were shown to hold for boron and magnesium where the radioactive elements produced were N_{7}^{13} and Si_{14}^{27}. The authors pointed out that it was highly probable that other bombarding particles such as protons and deuterons would also produce radioactive isotopes.

Convincing as were these results, it was necessary to exclude the faint possibility that the activity might be due to some kind of excited state of the bombarded nucleus rather than to a new and different element. So the Joliot-Curies set out to test whether the active elements were chemically distinct from original nuclei. In the case of boron they irradiated boron nitride and then treated it with hot dilute sodium hydroxide when it decomposed with the evolution of ammonia. They showed that the non-volatile residue was inactive but that the ammonia was active, with a 14 minute period. Similarly with aluminium: an irradiated sheet was dissolved in hydrochloric acid and evaporated to dryness when the residue was found to be inactive. The active element was shown to have combined with the hydrogen evolved to form a gaseous compound. When however the aluminium was dissolved in aqua regia then no activity was found in the gas. Now it was known that phosphorated hydrogen, PH_3 behaves in just this way, that is, it is formed with HCl but not with a mixture of HCl and HNO_3. Thus the presumption that the active element was indeed an isotope of phosphorus was strong. The chemical operation took only 3 min, compared with a decay period of 3 min 15 s for the radioactive aluminium. This beautiful chemical proof that transmutation had occurred was published on 5 February 1934, three weeks after the first paper.

This great discovery of artificial radioactivity was recognized in the following year, 1933, by award of a Nobel Prize for Chemistry jointly to Irène Curie and Frédéric Joliot; Joliot in his address at Stockholm drew attention to the possibility of chain reactions involving neutrons. At this time their ages were 38 and 35.

It has always been an oddity of scientific history that the discovery of artificial radioactivity was not made many years before 1933. It certainly could have been if a determined search for it had been made with the normally available strong radioactive sources and sensitive electrometers. However, there is no doubt that the development in 1929 by Geiger and Müller of the tube counter named after them did greatly facilitate the measurement of weak beta activities. But it still remains strange that this instrument was not used for the search for induced radioactivity sooner than it was: there must have been many laboratories where this could have been easily achieved. In fact the Joliot-Curies' discovery of induced radioactivity arose out of two independent discoveries, that of the neutron and of the positron. Though the positron could well have been discovered many years before it was, if some simple and obvious experiment had been made, this is not so for the neutron. Here there was no clear signpost to the goal. Two chance discoveries had to come to pass and one of these depended on the availability of very strong sources of polonium. As the only radioactive substance which emits almost only alpha rays and no gamma rays or beta rays, it had a very great advantage over the much more readily available and stronger sources of radium and thorium active deposits which emit strong β and γ rays. These special properties of polonium were decisive for the discovery of the projection of light nuclei which would have been difficult with a big background of gamma rays from an active deposit source. However, for the discovery of artificial radioactivity, polonium was not at all necessary, since the source was removed before the intensity of the irradiated specimen was measured: so the experiment could have been made easily using active deposit sources. When the discovery was finally made by the Joliot-Curies it was simple, elegant, complete, and opened up a whole new field of physics.

The production of more new radioactive isotopes by the use of protons, deuterons and neutrons soon followed as the Joliot-Curies had predicted. An important technical basis of the discovery of induced radioactivity was their high skill in radiochemistry which owed much to the great school of Marie Curie. This skill was utilized both in the preparation of the essential polonium source and in the chemical identification of the minute amounts of active elements the number of atoms of which were not more than 100000. On the conceptual side Frédéric and Irène Joliot showed their usual flair for the simple and effective experimental arrangement, combined with keen physical understanding and great experimental skill.

The next few years saw many new researches by the Joliots and by their colleagues, and a number of these were in the ever-widening field of induced radioactivity. A useful aid to this work was the construction of a 1·2 million volt Van de Graaff machine, which when used to accelerate deuterons, gave sources of radioelements 100 times as powerful as those obtained with natural radioactive sources.

In 1937 Joliot was appointed to a professorship at the Collège de France, where he set about creating a centre of research in nuclear physics and chemistry: among his collaborators were Savel, Halban, Kowarski and Pontecorvo. The construction of a 7 MeV cyclotron was begun and some 20 publications from the laboratory appeared in the first year.

At the same time Joliot was occupied with the setting up of a new laboratory under the Centre National de la Recherche Scientifique (CNRS). This was named the 'Laboratoire de Synthèse Atomique' and contained departments of physics, chemistry and biology in close collaboration, and an animal house. Close relations were maintained between Joliot's two institutes and the Institut du Radium where Irène Curie continued to work.

At the end of 1938, Hahn and Strassmann in Berlin showed that uranium underwent fission under the action of neutrons. This discovery stimulated a vast output of researches from laboratories in many countries. Within a year, when the outbreak of the Second World War and the prospect of atomic explosives closed down publication in this field, over a hundred publications had appeared in the world's scientific journals. Joliot and his colleagues entered energetically into this new field and between January and October 1939 published eight papers (57 to 64). Nearly simultaneously with Frisch in Copenhagen Joliot showed that the energy of fission was of the order of 200 MeV, as was expected from the masses of the elements concerned. This was done by collecting the recoiling fragments from a thin uranium sheet under neutron bombardment and measuring their range in air, which was found to be about 3 cm. He also succeeded in taking the first cloud photograph of a fission fragment.

With Halban and Kowarski, he showed that about three fast neutrons were emitted when uranium suffered fission by a thermal neutron. This result led to the expectation that a diverging chain reaction should occur in a suitable arrangement of uranium and of a material to slow down the fast neutrons. Convergent chain reactions were produced in this way in the laboratory and the practical possibility of attaining a divergent, but controllable chain reaction in a heterogeneous arrangement of uranium and heavy water or graphite was clearly envisaged. Control by the use of absorbing materials such as cadmium was proposed.

Five patents were taken out covering various aspects of the problem of obtaining useful power from uranium fission, three being in the names of Halban, Joliot, Kowarski and Francis Perrin, and two, concerning the problem of the requisite heterogeneity and of the limits of the dimensions of the reacting system, were in the names of Halban, Kowarski and Joliot. These patents were given to the CNRS and later transferred to the Commissariat à l'Energie Atomique. If the whole field of nuclear energy had not been overwhelmed by military considerations, these patents might well have proved commercially valuable. At that time, that is towards the latter part of 1939, it is probable that Joliot and his colleagues had carried realistic thinking about the practical possibilities of extracting useful power from uranium farther than any other group in the world.

Shortly before the defeat of France in 1940 Joliot had succeeded with the help of M. Raoul Dautry, then Minister of Armaments, and whom Joliot had told of the possibility of atomic explosives, in acquiring from Norway the whole of the world's stock of heavy water for experiments on reactor systems. He also obtained from Belgium 7 tons of uranium oxide. When the fall of Paris was imminent the uranium oxide was hidden to be recovered after the war and used for the first French experimental pile in 1948; and Joliot arranged for the heavy water to be smuggled to England in the charge of Halban and Kowarski.

In the British Government statement, issued on August 12 1945, immediately after Hiroshima and Nagasaki, the work of Halban and Kowarski is referred to in the following paragraph:

'These two French physicists had been sent by Professor Joliot to this country at the time of the fall of France in June 1940. They brought with them the 165 litres of heavy water—practically the whole world stock of this material—which the French Government had bought from the Norsk Hydro Co. just before the invasion of Norway. Drs Halban and Kowarski were instructed by Professor Joliot to make every effort to get in England the necessary facilities to enable them to carry out, with the co-operation of the British Government, and in the joint interest of the Allies, a crucial experiment which had been planned in Paris and for which the heavy water had been acquired. Facilities were provided at the Cavendish Laboratory, Cambridge, and by December 1940 they produced strong evidence that in a system composed of uranium oxide (as actually used) or uranium metal with heavy water as the slowing-down medium, a divergent slow-neutron fission chain reaction would be realized if the system were of sufficient size. It seemed likely that, if uranium metal were used, this critical size would involve not more than a few tons of heavy water.'

In October 1939, a few weeks after war broke out, Halban, Joliot, Kowarski and Perrin had published a paper in the *Journal de Physique et du Radium* entitled 'Mise en évidence d'une réaction nucléaire en chaine au sein d'une masse uranifère' (64). On 30 October Halban, Joliot and Kowarski deposited a sealed envelope with the Academy of Science. This was opened in 1949 and the enclosed paper by Halban, Joliot and Kowarski entitled 'Sur la possibilité de produire dans un milieu uranifère des réactions nucléaires en chaine illimitée' was published in the *Comptes Rendus* in November 1949 (75). The former paper gives an account of the following experiment. A copper sphere 50 cm in diameter was immersed in a bath of water and was itself filled either with water, 300 kg of powered uranium oxide U_3O_8 or a given mixture of oxide and water. At the centre of the sphere was placed a neutron source consisting of 1 g Ra and 160 g Be. The intensity of the neutrons was measured by the activity of a dysprosium detector placed in the sphere or in the water outside, so giving the curve $I = f(r)$, where r is the distance from the centre. This curve was measured with the sphere filled with water, dry oxide and various mixtures of oxide and water. A detailed discussion of the rather complicated results led the authors to the conclusion that the number of observed neutrons was over twice as many as that which could be expected to result from primary fission, assuming the already demonstrated value of $3{\cdot}5 \pm 0{\cdot}7$ neutrons per fission by thermal neutrons. Consequently they concluded that it was necessary to assume that a convergent chain reaction was taking place.

In the second paper, kept secret at the time as described above, the condition for a divergent chain reaction in a homogeneous system containing only U, H and O, is given as

$$(1-a)\ (1-p)\ (1-r)\ (1-s)\gamma > 1$$

where a is the mean probability of escape of a neutron by diffusion from the system, p is the probability for absorption by resonance in an infinite system, γ is the mean number of neutrons produced at a fission process and

$$r = \frac{C_H \sigma_H}{C_H\sigma_H + C_U(\sigma_{U\,\mathrm{cap}} + \sigma_{U\,\mathrm{rup}})}$$

and

$$s = \frac{\sigma_{U\,\mathrm{cap}}}{\sigma_{U\,\mathrm{cap}} + \sigma_{U\,\mathrm{rup}}}$$

when C_H and C_u are the concentration and the various cross-sections are designated by σ_H etc.

Using the best available data the authors conclude that it is almost certainly impossible to obtain a divergent chain reaction in a homogeneous reactor containing hydrogen and uranium in their natural isotopic ratios. They held that an enrichment of as little as 1:1·2 in the ratio of ^{235}U to ^{238}U would suffice to allow a divergent reaction to proceed. They also discussed the alternative method of using heavy instead of ordinary water. Finally they showed that it might be possible to obtain a divergent chain reaction with a mixture of natural uranium and natural water, provided that the two were heterogeneously distributed. This is now known not to be the case. They suggested spheres or cubes of a hydrogen-containing material such as paraffin, distributed in a mass of uranium, and that the size of the spheres should be intermediate between the diffusion distance of a thermal neutron before capture by hydrogen and the free path of a neutron in the energy range for resonance in uranium. These considerations suggested that with hydrogen spheres with a diameter of a few centimetres, distributed in the oxide of uranium so as to give a global composition of about 1 hydrogen atom to 1 uranium atom, a divergent chain reaction should be possible. It was pointed out that this would be much easier to achieve if deuterium-rich water was used instead of natural water.

There is little doubt that, had the war not intervened, the world's first self-sustaining chain reaction would have been achieved in France.

All work on chain reactions in uranium stopped with the occupation, when the German military authorities took over Joliot's institute. In September 1940 a meeting took place in Joliot's office at the Collège de France between M. Faral, representing the administration of the Collège, and Professor Erich Schumann, representing the German military authorities. Joliot agreed to continue as Director of the Laboratory of Nuclear Chemistry under the condition that no work of military character should be carried out by the group of German scientists which the German Administration intended to set up in the Laboratory, particularly to work with the cyclotron.

Soon after this W. Gentner, who had worked in Joliot's Institute from 1932 to 1934, was sent as interpreter with a German military mission to Paris, and amongst other things to enquire into the whereabouts of Joliot's heavy water and uranium. However the former was already in England and the latter was safely buried. Gentner arranged a secret meeting with Joliot and asked him if he would like him to become leader of the German group at the institute: Joliot replied that he would prefer him to anyone else. With the help of Professor Bothe, Gentner managed to get himself appointed head of the German physics group with a written agreement with the German authorities that no research for military purposes should be done at the Institute. This agreement became most useful to Gentner when on two occasions Joliot was arrested by the Gestapo and Gentner, aided by the military, successfully intervened to get him released. Writing of this period M. Pierre Savel, lifelong friend and collaborator of Joliot, wrote of Gentner's 'aide précieuse pour Joliot et pour nous tous'.

In 1943 Gentner was recalled to Germany because of his 'weakness' and a new group of German scientists were installed under the direction of National Socialist diehards. Several attempts were made by them to use the cyclotron for their own purposes, but it always happened that technical faults developed and the Germans never succeeded in getting any experiments carried out.

In 1944 Joliot arranged for Langevin to escape to Switzerland and he himself had for a time to go into hiding. In 1941 Joliot had been concerned with the foundation of the 'Front National de Lutte pour la Libération et l'Indépendence de la France', one of the main bodies organizing the Resistance, and became its first President. During most of the war radio receivers and transmitters for the maquis were constructed in Joliot's laboratory and towards the end explosives were also manufactured.

During these years Joliot continued such scientific work as was possible with the limited means at his disposal. His 7 MeV cyclotron had now been finished, but was not available to him as it had been sealed by the German military. With his wife he studied the fission of ionium and measured the cross section relative to that of uranium and thorium, and they continued in a small way the study of the fission products of uranium, developing methods of separating and identifying them. Later Joliot became active in the field of biophysics and collaborated with M. Sue of the chemistry section of his laboratory at the Collège de France, and with a biologist, Professor Leblond, in the study of the metabolism of iodine, employing radioiodine. With M. Feyel and M. Sue he studied the permeability of the walls of the red corpuscles of the blood to sodium iodide, etc. With M. Robert Courrier, Professor of Endocrinology at the Collège de France, some important experiments were made of the role of iodine in the functioning of the thyroid gland, and in particular of the role of thyroxine and di-iodothyrosine. With M. Lacassagne he succeeded in producing a cancer in the liver of a rabbit by means of neutrons. As a result of this contact with the biological disciplines, Joliot formulated his views as to the methods of collaboration in the following passage:

> 'D'une façon générale, ces recherches en biologie nous ont montré combien est nécessaire la collaboration étroite des chercheurs de discipline variées, se complétant l'une l'autre. Il ne s'agit pas pour le Physicien de donner des conseils sur telle ou telle méthode de mesure physique, mais de travailler effectivement sur le sujet biologique, sans prétendre à la compétence dans ce domaine, mais en vue d'apporter une forme de pensée différente, s'ajoutant efficacement a celle, fondamentale, du biologiste.'

In August 1944, immediately after the liberation of France, Joliot was appointed Director of the Centre National de la Recherche Scientifique and, till January 1946, he was almost entirely concerned with its organization. During this period much was accomplished in the task of getting French science going again: among many other things an institute for nuclear physics at Strasbourg was created. Towards the end of 1945, Joliot recommended to the French Government that an organization be set up to take up the work of nuclear physics and its applications to technology which had been interrupted in 1940. As a result the Government created the 'Commissariat à l'Energie Atomique' under the direction of Joliot as High Commissioner and M. Raoul Dautry, then Minister of Reconstruction, as Administrator-General. It had been Dautry, who, as Minister of Armaments in 1940, at Joliot's suggestion, had acquired the world stock of heavy water from Norway. The other Commissioners were Irène Joliot-Curie, Francis Perrin, Pierre Auger, and General Dassault representing the Defence Ministry. Several French scientists, who had participated in the Anglo-Canadian Atomic Energy project, joined the organization, among them being Kowarski, Goldschmidt and Gueron. Together with many research workers from Joliot's own laboratory, a research station was quickly started in an old military fort at Chatillon on the outskirts of Paris and within two years, that is in 1948, the first French pile ZOE came into operation. This was a heavy water pile using uranium oxide and operating at up to 1000 kW. The next step was to start a very powerful new research centre for all branches of nuclear research and technology at Saclay about 10 miles south-west of Paris. Here were installed a 280 ton cyclotron, a 5 MeV Van de Graaff accelerator and a second pile, using graphite and uranium metal, with an output of 10000 kW. In the first thirteen years of its existence, 1946 to 1959, the French Atomic Authority has made spectacular growth and is the fourth largest in the world. It is planned to produce a quarter of France's electrical power from nuclear stations by 1975. In 1959 a 3 GeV proton synchrotron, Saturne, similar to that at Brookhaven in the U.S.A. started operation at Saclay and British workers are being invited to make experiments with it. This is the first machine in Europe, excluding the U.S.S.R., to produce strange particles.

In 1946 Joliot was elected a Foreign Member of the Royal Society of London and in 1947 was awarded the Hughes Medal.

When in January 1946 the General Assembly of the United Nations brought into being the Atomic Energy Commission, Joliot became a member of the French delegation, together with Pierre Auger and M. Parodi as leader. They were long in New York engaged in the discussions on the possible methods of control, arising out of the Lilienthal and Baruch Plans. Joliot also played an important part in the founding of Unesco and helped it through its early years.

In 1950 Joliot was dismissed from the post of High Commissioner of the Atomic Energy Authority on the grounds of his membership of the French Communist Party and was succeeded by Francis Perrin.

After he had left the Commissariat of Atomic Energy, he still retained a keen interest in its problems. In a speech made in 1954 to a society of industrialists, Joliot discussed very frankly the then somewhat disappointing state of physics in France, in comparison both with the state before the war and with the state that could, he felt, have been reached if the excellent younger generation of French scientists had been better supported. He particularly emphasized the paucity and low performance of the accelerating machines in France in especial contrast with those in Great Britain, a country he remarked as being of comparable economic strength. Since Joliot made these strictures some definite advances have been made, in particular the very successful machine Saturne at Saclay has come into operation.

In 1951 Joliot returned to full-time direction of both his laboratories, that at the Collège de France and that under the Centre National de la Recherche Scientifique: he continued to direct a large number of research workers in both these laboratories till his death in 1958. Ever since the war, in spite of his heavy administrative duties, Joliot had continued his lecture courses at the Collège de France, where the regulations insist that each professor must give a new course every year: during his tenure he recalled that he gave thirteen different courses of lectures.

His wife Irène Joliot-Curie died in 1956 and Joliot was appointed in her place as Professor of the Faculty of Science of Paris and Director of the Curie Laboratory of the Institute du Radium. In 1957, recognizing the need for more space and greater facilities for nuclear research than could be provided at his laboratories in the centre of Paris, he began the construction of a large new centre at Orsay, south of Paris. This was called 'Les Laboratoires de Physique et Chémie nucléaire de la Faculté des Sciences' and was financed by the CNRS. Amongst other equipment a 1000 MeV linear electron accelerator is now being constructed. The laboratory was just coming into operation at the time of Joliot's death.

From his earliest days Joliot was keen on sport and athletics, including Judo, tennis, skiing, sailing and above all sea fishing. Marie Curie had built a small house at Arcouest in Brittany and the Joliot-Curies spent most summers there with a sailing boat and with their two children and later grandchildren. Each winter they went to the Alps—Irène had long been an accomplished alpinist. Both their children have become scientists, Hélène born in 1927 became a nuclear physicist and Pierre a biophysicist. Hélène became the wife of Langevin's grandson, named Michel, thus uniting in marriage the families of the two old colleagues Marie Curie and Paul Langevin.

Joliot had debonair good looks, was gay and direct in manner and an excellent lecturer with a fine voice and command of language. In his later political days he found he also possessed the gift of mass oratory and could hold an open-air audience of hundreds of thousands of people.

Joliot's social and political interests began effectively after the rise of Hitler and the abortive coup in Paris in 1934, when he was drawn by Langevin into the organization of anti-fascist intellectuals. Langevin himself was all his life a radical in politics—he came of a Parisian working class family—and he believed passionately in the possibility of human betterment which could result from the rational application of modern science. Joliot had a considerable share in the foundation of the CNRS a few years later and was a member of its Council from the start. When the war came it was his profound feeling for France and for French culture which led him in 1940 to remain in Paris to carry on the fight against Hitler from there, while at the same time he encouraged and helped many of his colleagues to escape to England and America. It was his experience of his country's defeat and occupation and his work in the Resistance which led him in 1942 to join the French Communist Party; in 1956 he became a member of its Central Committee.

Joliot's ardent support of Communist policy inevitably resulted in some estrangement from some of his scientific colleagues. This came to a head at the time of his dismissal from the post of High Commissioner of the Commissariat à l'Energie Atomique in 1950. This took place not only at a time of great political tension resulting from the Korean War, but at a period when the French Government changed the original object of the French atomic organization which initially was to be concerned solely with the industrial and scientific aspects of atomic energy, and decided to make atomic bombs. Just at this time also in the United States a situation of great personal difficulty existed for many prominent atomic scientists, since the explosion of the first Soviet atomic bomb in 1949 had precipitated the struggle about the H-bomb, and involved them deeply in sharp controversy over many of the highly complex scientific, military and political aspects of atomic energy.

Some of the environmental conditions and innate qualities which played a major role in shaping Frédéric Joliot not only as one of the outstanding experimental physicists of our time, but as the most notable French scientist and scientific administrator of his generation, are not hard to discern. He was lucky in his first teacher Langevin, the foremost French theoretical physicist of his day, who possessed a mind of exceptional lucidity and the gift of reducing a complex physical phenomenon to its simplest elements. Then came the experience of working in Marie Curie's famous laboratory with its great tradition not only of disciplined, painstaking and precise investigations, but of the discovery of unexpected new phenomena. Commenting much later on his early days as research worker, he wrote: 'Ce fut sous la direction d'un maître exceptionnel, Marie Curie, qui j'eus la chance de faire mon apprentissage de chercheur dans un domaine de la science merveilleusement attachant et vivant.' Commenting on some aspects of the apparatus with which he and his wife discovered the projection of light nuclei by the rays from a polonium and beryllium source, which led directly to Chadwick's discovery of the neutron, Joliot wrote: 'Certes, il faut partir d'une idée préçoncue mais chaque fois que cela est possible, l'expérience doit être montée pour ouvrir le plus de fenêtres possible sur ce qui n'est pas prévue. "Qui peut le plus, sans gêne, peut le moins." ' This expresses admirably the long-held view of the writer, but in a much more succinct manner than he has ever achieved. This principal was clearly exemplified in the greatest of Joliot's discoveries, that of induced radioactivity. Another interesting comment by Joliot on the art of experimentation referred to the occasional role of flashes of intuition—'ces eclairs de pensée'—as to the right way to proceed. 'J'ai eu des brusques illuminations me fournissant la meilleure manière de produire ou d'observer un phénomène, avec la sensation immédiate que la méthode ainsi suggérée était unique et que toute autre serait moins simple. Cette sensation est rare, et je me rappelle au moins dans deux circonstance'. Much of Joliot's experimental work certainly had the characteristic of simplicity and directness. A French colleague wrote of him: 'Esprit clair et methodique il avait le don d'imaginer rapidement des expérience simple qu'il exécutait avec grande ingéniosité et habilité.'

The relative part of the two lifelong colleagues Irène and Frédéric in their great record of joint work is not easy to identify, which suggests, what is probably the truth, that this was a real collaboration of very similar aptitudes: possibly Irène may have been the better chemist and Frédéric the better physicist—but this is by no means sure.

With the development of accelerating machines for nuclear particles, Joliot was not slow to realize that the natural radioactive sources, on which all nuclear physics had previously depended, would soon be superseded by machine-made sources. The 7 MeV cyclotron installed in his laboratory at the Collège de France just before the war was the first of such machines on the continent of Europe, excluding the U.S.S.R. When in 1944 he became Director of the CNRS, he energetically set about the task of providing French science as a whole with the necessary tools, without which the revival of French science to something nearer to its former glory could not be hoped for. In this task he achieved much, but not as much as he had hoped. Then from 1946 to 1950 he devoted all his energies to the building up of the French atomic organization and in this achieved great success. From 1944 to 1950 Joliot was without doubt the dominating figure in the higher administration and planning of French science.

For help in the writing of this memoir, I am greatly indebted to the Joliot-Curies' daughter, Mme Hélène Langevin and to Professor Savel of the Collège de France. I also wish to thank Sir John Cockcroft for his help and advice.

P. M. S. Blackett

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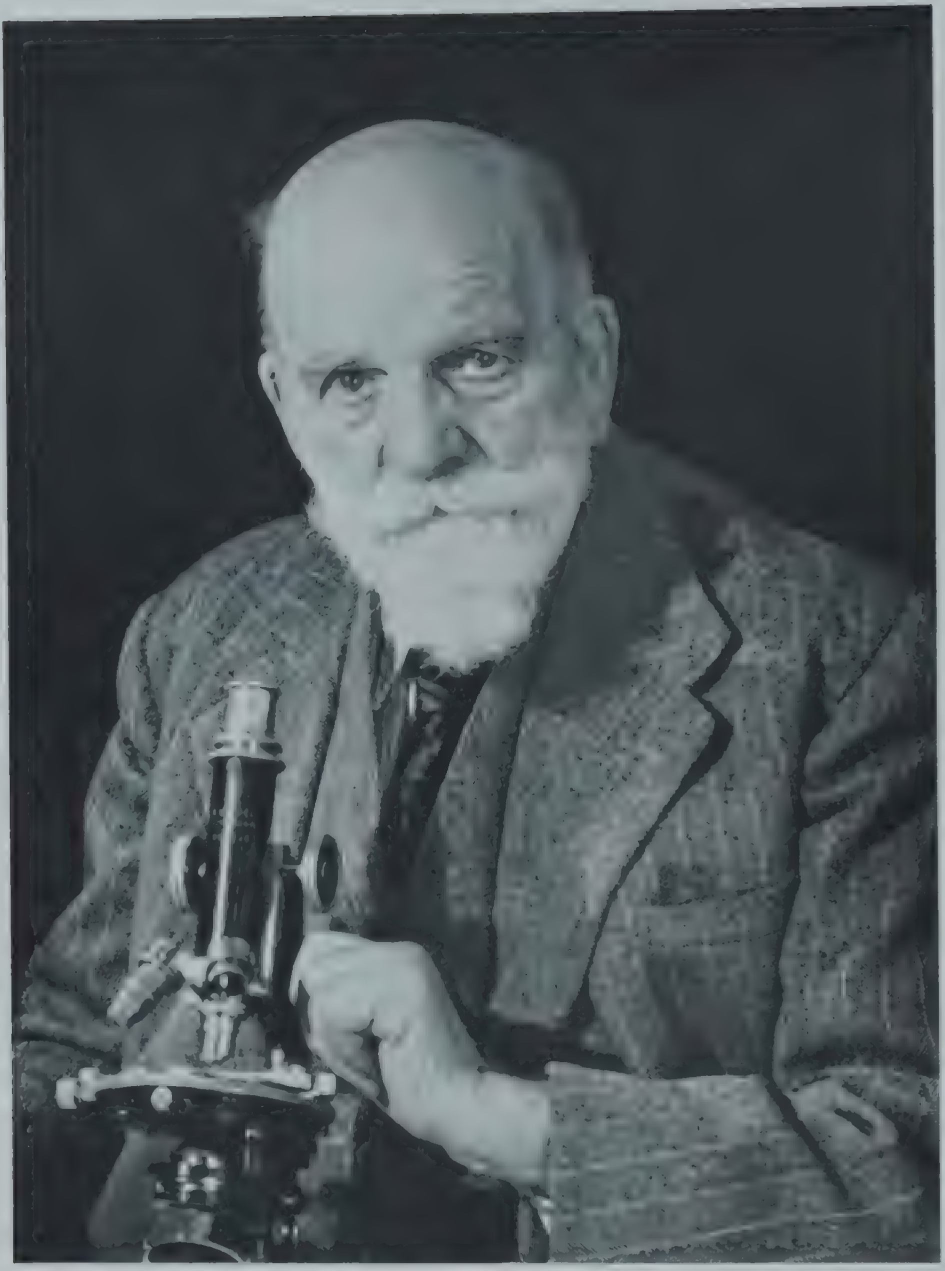

K. Jordan

HEINRICH ERNST KARL JORDAN

1861-1959

Karl Jordan, affectionately known for very many years by those most closely in touch with him as 'K.J.', was born the youngest of seven children of farming stock in the small village of Almstedt near Hildesheim in Hanover on 7 December 1861. As a child he had to help on the farm, yet found time to indulge a lively interest in the wild life of his surroundings. At the age of five he lost his father, and but for the generosity of an uncle who enabled him to attend the Hildesheim High School, his massive contributions to the study of entomology might never have been forthcoming. In 1882 he entered the University of Göttingen and four years later took his degree in botany and zoology, passing *Summa cum Laude*. Two years later, after securing a teaching diploma and putting in a year's military service (not without hardship), he was appointed a master at Münden Grammar School. Here he remained five years and here it was that he became known to two men whose acquaintance was to shape his whole future career. One was A. Metzger, Professor of Zoology at the Academy of Forestry, the other Count Berlepsch, an enthusiastic amateur ornithologist. In 1891 he married Minna Brünig with whom he had fallen deeply in love whilst both were still in their teens. It proved a profoundly satisfying marriage, broken only by her death in 1925. Although he quite rapidly acquired a British, indeed an international outlook, she remained to the end German, yet stoically bore the distresses this caused during the 1914-1918 world war.

Hardly had Jordan taken up his duties as teacher of mathematics, physics and natural history at the School of Agriculture in Hildesheim, in 1892, than, through the strong recommendation of his two good friends he received from Dr Hartert (recently appointed Director of Walter (later Lord) Rothschild's Museum at Tring) an invitation to join the staff of that Museum as entomologist. As Miriam Rothschild has written (Rothschild, M., 1955, p. 2): 'It is difficult to envisage a queerer state of affairs than that which K.J. found at Tring when he arrived there in 1893. Walter Rothschild was then twenty-four years of age and a most eccentric figure. He was always just about to leave the Rothschild banking house in disgrace, not for any serious misdeed, but because he displayed a quite remarkable lack of business acumen . . . and—worst of all—while pretending to attend the House of Commons actually spent all his time at the Natural History Museum . . . Another equally disconcerting aspect of the situation at Tring was the complete lack of facilities for working with a microscope. Walter Rothschild's father had recently erected two cottages on the outskirts of the park in which his son was supposed to house his museum, but there was no space to accommodate the beetles which K.J. was engaged to "arrange, determine and classify". This gigantic overflow was stacked in boxes at random and in fearful confusion, in sheds and hired rooms scattered about Tring. K.J. had not even a suitable desk at which to work, and found it necessary to retire to the landing window-sill if he required sufficient light by which to use so much as a powerful lens. But this weird state of affairs in no way disconcerted him; with characteristic objectivity he appreciated the immense opportunities Tring had to offer. Here was the chance for both interesting and creative work. He took it.

'It is an almost incredible fact that within a year the collections of over 300 000 beetles (60 000 species) were arranged and classified—in the more conspicuous families named down to genera and species—while by the end of the following year over 400 new species had been described. K.J. was quick to realize that one aspect at least of Walter Rothschild's mania for size was both important and valuable. It was only a large collection such as this, containing both a great number of species and very long series of specimens, which could give him the facts he required.'

Normal hours of work were far exceeded. Living only a stone's throw from the Museum Dr Jordan quickly contracted the habit, which stayed with him till after his ninetieth birthday, of working on in the Museum after dinner well into the night. For recreation, he would take long walks or cycle rides and, later, took to gardening—the writer of this note well remembers seeing him vigorously wielding a pick in a recalcitrant corner of his rock-garden at the age of 91.

Dr Jordan's incredible industry is at no period better exemplified than by the product of his first few years at Tring. Between 1893 and 1895 he not only reduced the chaotic insect collections to order but saw the first instalment of the *Revision of the Papilios of the Eastern Hemisphere*, consisting of some 300 pages, actually published. True it is that the main text of this very important work was by Rothschild, but there are many notes by Jordan who also provided all the drawings of anatomical details. Much light is shed upon the nature of the collaboration between Rothschild and Jordan by Jordan himself in his tribute to Lord Rothschild published in 1938 (*Nov. Zool.* **41,** 1-14), from which the following passages are quoted. 'Walter Rothschild worked in many instances quite independently, neither discussing the subject with the curators, nor submitting the manuscript' . . . he . . . 'had never taken kindly to the microscope and microtome. Structural details easily escaped him. Like an artist, he perceived the animal as a whole and not the details . . . which would have been revealed by the cold lenses of a microscope.' Yet one of the outstanding features of all their joint work, as of Jordan's alone, is the insistent attention paid to structural details, no few of which were utilized by them for the first time.

Jordan's introductory note to this first major product of the Tring Museum in the field of entomology is of particular interest as it sets out for the first time the taxonomic concepts and general principles which guided all his work, with but relatively trifling modifications and improvements in later years. As Eugene Munroe has so aptly written (Munroe, E., 1955, p. 69): 'Jordan's notes on *Papilio*, written ten years before the rediscovery of Mendel's laws, and forty years before the modern synthesis of taxonomy and population genetics, can be read virtually without reservation to-day. Here we find emphasis on the importance of series and, more, of adequate samples from every part of the geographic range. Here is a worker in possession of hitherto unrivalled material, who might well have been excused a touch of complacency, but who instead borrows specimens from almost every important collection, and bemoans the inadequacy of the accumulated resources of the world. Here, too, we find a clear understanding of the importance of types as standards of identification; here is a perfectly modern concept of the population as the basic unit in taxonomy, and a clear relegation of environmental and individual variation to a subordinate place; here is recognition of the plasticity and local variation that exist within larger and more definitely characterized populations; here are a mature appreciation of the process of geographic speciation, a firm and definite subspecies concept, and a recognition of the importance of vicarious species. These clear ideas are reflected in an equally clear nomenclatorial practice: the trinomen is explicitly reserved for the subspecies; indeed we find Jordan urging the taxonomic co-ordinacy of the nominate subspecies with other subspecies, and its nomenclatorial treatment as a trinomen, on his as yet reluctant co-editors, Rothschild and Hartert!' Here for the first time in lepidopterology, as Jordan has himself remarked (1938, *Nov. Zool.* **41,** 9), geographical races described by various authors as distinct species were almost consistently (as much as the material at hand warranted) reduced to the rank of subspecies and the classification thereby much clarified.

Miriam Rothschild remarks how quickly one grows accustomed to scientific advances and how easily one takes them for granted. It will perhaps help to put Jordan's views in perspective by remembering that they were elaborated at a time when scorn and derision were still being poured on the theory of mimicry; when the occurrence of polymorphism such as exists in the African swallow-tail butterfly *Papilio dardanus* was still regarded by many leading entomologists as an impossibility; and when A. G. Butler in the British Museum and F. Moore in India had barely stopped describing as new species large numbers of *Terias* which, according to Jordan's concepts, could only be regarded as, and in fact were, seasonal forms or poorly differentiated geographical races. One must remember, however, that in those days the truth of another of Jordan's dicta was not generally realized: two or three specimens were considered adequate material on which to base a 'species': the need for long series, more and ever more material, was constantly stressed by Jordan, even though the material available to him at Tring was outstandingly greater than that of any other museum or private collection then in existence. Lord Rothschild once remarked to the writer that 'he never had any duplicates'!

Much has been heard during the past two or three decades of the 'new systematics' born of the discovery of Jordanian systematics by followers of other biological disciplines, notably the geneticists. Throughout his long career Jordan was a forthright champion of what this writer prefers to call the true systematics; and he could be scathing. 'Classification, as we know, has the reputation of being as dry as our cabinet specimens . . . and of interest only for those who work on the special group of animals classified. There are even biologists of fame who, in their misguided wisdom, scoff at systematics and look down upon this kind of work as more or less fruitless . . . I take the opportunity . . . of stating emphatically that sound systematics are the only safe bases upon which can be built up sound theories as to the evolution of the diversified world of live beings' (1911, *Proc. Int. Ent. Congress*, p. 385). It is curious that neither here (which would be rather early for it) nor later does Jordan make any reference, or only the most cursory references, to the work of the geneticists. Its possibilities can hardly have escaped him. In later years he often spoke with appreciation of the evolutionary geneticists and with regret that their important fields had not been opened up earlier.

Three orders of insects provided Jordan with the basis of all his taxonomic work, the butterflies and moths (Lepidoptera), the beetles (Coleoptera) and the fleas (Siphonaptera). Beetles attracted him most and, among these, the Anthribidae, a large family of 'long-horned' weevils displaying a truly bewildering range of variation, fascinated him. Over the years he published 145 papers on this single family and described 150 genera and some 1900 species and subspecies, representing nearly half the known genera and two-thirds of the known species. All this, however, he regarded quite properly as merely preliminary work essential for the provision of a sound classification of the family, remarking that it would have saved taxonomists an enormous amount of laborious drudgery if Noah had 'imprinted an inheritable name on the bodies of the millions of species he saved from extinction by drowning'. Jordan could always see both the wood and the trees, the immediate needs and the distant objective. At the very end of his career he was still seeking logical explanations of the seemingly chaotic riot of variation displayed by these, his favourite, beetles. One is tempted to wonder why, with so much material so richly endowed with features well calculated to arouse speculation on evolution, taxonomy and cognate matters, Jordan hardly ever broached these subjects in any of his writings on Anthribidae. The reason is almost certainly that which confronts every taxonomic entomologist, no matter what his group, with a few exceptions: however extensive his material, it is hardly ever more than a small fraction of what he needs as a basis for satisfying work.

About the turn of the century Lord Rothschild gradually parted with all his Coleoptera, except the Anthribidae, so channelling his entomological curator's activities towards the Lepidoptera, in which he was especially interested, altering the nature but by no means reducing the volume of Jordan's output. Of all the insects, the Lepidoptera are undoubtedly the most familiar and most collected. And of all the Lepidoptera by far the best known sixty years ago, as now, were precisely those groups which Rothschild, with Jordan's help, decided to monograph—the swallow-tailed butterflies, the hawk moths and the Charaxidini. Fairly abundant material of these was available (though it entailed searching most of the museums and private collections of Europe), and something was known of the bionomics of a good many of the species. The *Revision of the Eastern Papilios*, already referred to above, was based on the examination of some ten thousand specimens distributed amongst the 239 species which the authors recognized. This was followed by *A Monograph of Charaxes and the allied prionopterous genera*. The four parts of this appeared in 1898, 1899, 1900 and 1903, were profusely illustrated and ran to 375 pages. It is astonishing that simultaneously (in 1903) the *Revision of the lepidopterous family Sphingidae*, a work of over 1000 pages, illustrated by 67 plates, also was published. Sheer volume provides no criterion of value but when the quality of these works is considered, their production in so short a space of time is little short of phenomenal. In originality, breadth of outlook, accuracy and sound judgment they far surpassed all previous publications of a like nature, and little of a similar comprehensive character and permanent value has been published since. These are still the standard works on the subjects of which they treat; only here and there have modifications had to be made in the light of later discoveries.

It was not until 1906 that Rothschild and Jordan brought out the second part of their projected monograph of the swallow-tailed butterflies, dealing with the American Papilios. In many ways this was a greater achievement than their treatment of the oriental species, for the state of confusion that had till then reigned in the group was greater, the available material and information much less and the taxonomic problems more complex. Moreover, owing to the nature of the South American continent, material was available only from a limited number of points, such as the neighbourhood of the larger towns, and the banks of the rivers. The intervening areas were unknown. In spite of this Jordan was able to show on morphological grounds that many 'species', recorded from these widely separated areas, were truly only subspecies. He returns again, in the introduction, to his definitions of varieties, rather unnecessarily labouring the point one would say nowadays, until it is remembered that at that time the acceptance of his views and their expression in appropriate scientific terms and forms of nomenclature was still far from universal. Incidentally, he calls attention to a fact that must be little known to zoologists, namely, that it was Esper in his essay *De varietatibus* (1781, p. 18) who first introduced the term *subspecies* and defined it as 'that kind of variety which is an incipient species'. 'Therefore', says Jordan, 'the term subspecies is employed by us for nothing else but the geographical variety' since 'according to our researches the incipient species is represented by the geographical race . . . no other variety forms the basis of the development of a species into several species'. In the matter of the nomenclature of infrasubspecific varieties, he champions a system which almost inevitably will one day be adopted, under which each peculiarity will be known 'by a special morphological term'. He points out the inconvenience, if not the absurdity, of calling, for example, the uniformly black variety of one swallow-tail ab. *niger*, of another ab. *wilsonae*, of a third ab. *schmidtii*, and so on. 'The describing and cataloguing of "species" are certainly the basis of systematics, but also the lowest degree in this science. After that comes classification, or in other words, research in relationship.'

Simultaneously, during this period of the monographs, Jordan published numerous shorter papers describing the new discoveries which Rothschild's collectors were constantly making in the East Indies and particularly New Guinea. This activity, which Jordan always regarded as rather menial, however essential, continued into the 1950's but with the emphasis shifting gradually to the Anthribidae and Siphonaptera. At the same time several papers of a purely morphological nature appeared from his pen; for example, on the antennae of butterflies (1898), on the mesosternite of butterflies (1902), on a peculiar sexual character found among Geometridae (1905), on the basal spine on the forewing of Saturniidae (1911). Much of this work, however, is hidden in longer taxonomic papers, preparation of which necessitated it. It was almost exclusively Jordan's work on the Lepidoptera that provided him with the basis of all his more philosophical papers.

Jordan's interest in fleas dated from about 1898, or earlier, and was inspired by a schoolboy, Charles Rothschild, Walter Rothschild's younger brother. 'Before K.J. had won himself a desk on the ground floor of the Museum Cottage he was sometimes joined at the landing windowsill (his only work bench!) by a modest but very enthusiastic schoolboy for whom he instantly conceived a great liking' (M.R., 1953, p. 4). Elsewhere Miriam Rothschild records (1953, Hopkins, G. H. E. and Rothschild, M. *Cat. Fleas*, **1**, 5) how these two laid the foundations of the Rothschild flea collection through a successful setting of two traps one evening at Tring in search of *Typhlopsylla agyrtes*. Thereafter Jordan published, either alone or jointly with Rothschild, upwards of 140 papers on fleas, running to over 1450 pages and including descriptions of at least 75 new genera and 480 new species and subspecies. To illustrate this massive contribution Jordan prepared 1280 drawings, besides executing 375 others for papers published by Charles Rothschild alone. The uniformly high standard of all this work would again be regarded as quite remarkable, were it not that, after very little contact with Jordan's work, one learns to expect it. An admirable summary of Jordan's work on the fleas has been written by Robert Traub (1955, pp. 33-42). After calling attention to the many morphological features and important concepts in the fields of distribution, taxonomy and host-relationships to which Jordan was the first to draw attention, Traub writes: 'Probably Jordan's greatest contribution to the study of Siphonaptera has been the development of a system of classification of the higher categories, thereby ending literal chaos. This significant advance in systematics is based upon his sound background in morphology, coupled with an imaginative insight which enabled him to place new and fertile interpretations upon the relationships of genera, and the ability to consider new approaches as well as established methodology. The Order Siphonaptera seems unique concerning the enormous differences of opinion regarding familial assignment of major genera . . . The status of many other genera in the Order has been just as perplexing. The first taxonomic key which even attempted to cope with the many aberrant groups of fleas was that of Jordan in 1948, and this actually is a superb outline of the classification of fleas. Although by definition the key is not wholly inclusive, since the book dealt with arthropods of medical inportance, Jordan somehow managed to include in the key the great majority of the subfamilies of fleas. As a result, this chapter has been of incalculable assistance in the identification of fleas from remote areas since many exotic fleas simply would not fit anywhere in previous systems of classification. It cannot be overemphasized that the taxonomic characters used in this key were, to a great extent, wholly original and were not even mentioned by earlier workers, and are examples of Jordan's unusual interpretive powers. Among these original features are all save one of those listed for the superfamilies, and such structures as the vincula of the thorax, apical spines on the metanotum, and a transparent collar over the base of the anal tergum, all of which were formerly completely overlooked or disregarded. The path opened by the pioneer was readily followed by other workers, and Hopkins and Miriam Rothschild based their excellent classification upon that introduced by Jordan in 1948 and upon unpublished notes especially prepared for them at their request . . .

'The reader may be surprised that Jordan's epochal classification was not published until some fifty years after he first became interested in Siphonaptera. However, this apparent procrastination to a great extent explains why this scheme of systematics is so superior. Jordan deliberately waited as long as possible so that the maximum number of missing links and intermediate forms would be discovered. In the interim he went over the Tring collection many, many times, noting and studying every possible structure that might indicate relationship rather than similarities. This was enormously difficult, since so many fleas lack close relatives, and since similar morphological traits seem to occur spontaneously in apparently unrelated genera, while seemingly close forms often differ strikingly in a few particulars. Colleagues who visited Tring were astounded at Jordan's tremendous memory for detail, which enabled him to note an obscure characteristic in a species seen for the first time and then unerringly to point out, seemingly without effort, how the same feature occurred sporadically elsewhere in the order. Recitation of detail, however, did not interfere with his making sound generalizations as well. Only when all these pieces eventually began to fit in the puzzle did Jordan decide it was not premature to publish his ideas of classification.

'We are also much indebted to Jordan for exquisitely illustrating evolutionary trends within the Order Siphonaptera in 1950, and for simultaneously citing morphological details of use in determining the original affinities of this rather anomalous group of insects. Such an interesting and learned article could be written only by one who knew the comparative morphology of insects in general and was thoroughly familiar with the external anatomy of all types of fleas . . . This article is crammed with expert generalizations posing questions that will stimulate the students of the Order for years.'

An oft-told story, in which Dr Jordan played a leading part is that concerning plague and the systematics of the genus *Xenopsylla*. It is well told by Traub. 'In 1926 he pointed out that the Advisory Commission on Plague . . . assumed that *all* pale fleas were *X. cheopis*, the vector of plague. One reason why the theory of transmission of plague by *X. cheopis* met opposition was that there was no plague in areas such as Madras where *X. cheopis* was common. However, as long ago as 1908, Jordan and Rothschild theorized that more than one species of fleas had been present in the experiments undertaken by the Commission. It remained for N. C. Rothschild to demonstrate that there were two other *Xenopsylla* common on rats in India, *X. astia* and *X. brasiliensis*, and Hirst then speculated that the distribution of these fleas might be connected with the distribution of plague. The Tring specialists thereupon showed that the "pale flea" in the non-endemic areas was generally *X. astia*, an inefficient vector of plague. A similar condition occurred when plague authorities in Africa identified the local vector as *X. astia*. It seemed strange that *X. astia* could be an inefficient vector in India and a highly efficient one in Africa, but the mystery was solved when Jordan and Rothschild proved that these African *X. astia* were in reality *X. nubicus*.'

In concluding his analysis of Dr Jordan's work on fleas, Traub emphasizes that the papers written by him when he was in his eighties and nineties were 'the finest he has ever done as regards concepts of classification and relationships of fleas . . . the only truly satisfactory classification of fleas was published during this period'. There cannot have been many zoologists who at this age could still utilize their accumulated knowledge and experience with unabated mental ability.

Early in his career at Tring Jordan had been impressed by the comparative isolation of entomology from the main stream of zoological thought and activity. His broad training rebelled at the attitude to entomologists held by so many professional zoologists of standing who should have been well aware of the contribution which entomology could make to the science. This intellectual isolation of entomology from the rest of zoology was, however, accompanied at that time by an insularity, one might call it, amongst entomologists themselves. In his travels about Europe Jordan discovered that very few of the scientific men he met were personally known to each other, and that, as often as not, their attitude to one another was competitive rather than co-operative. The size of the entomological field, too, holding more than three-quarters of all the known species of animals in the world, was of itself a factor tending to split entomology into a number of subsidiary 'camps', the occupants of which hardly knew each other either.

In an effort to remedy this state of affairs, Jordan conceived the plan of organizing a series of international entomological congresses, inspired perhaps by the zoological congresses which he had attended at Cambridge and Berlin. Little of his early world-wide correspondence on this project now exists, but in one of these early letters (from Bengtsson) there is the illuminating sentence, 'Es ist leider so gewiss, was Sie sagen, dass unsere schöne liebe Wissenschaft ein Stiefkund der Zoologie geworden ist und dass die Stellung der Entomologie nicht der Wichtigkeit der wissenschaftlichen und praktischen Ergebnisse derselben entspricht'. To cut a long story short, the combination of Jordan's industry, tact and sound practical good sense, inspired the Belgian entomologists enthusiastically to take up the idea and to organize the first Congress in Brussels in 1910. The second, Oxford 1912, was only saved from last-minute collapse, through the illness of the secretary, by the personal intervention of Jordan himself. From then on Jordan was the king-pin, in fact, if not always in name, the permanent secretary of the organization and twice resurrected it from the ruins of war. In 1948 at the Stockholm Congress, when too deaf to take part effectively in discussion, but still more audible than the great majority of speakers, he was elected Honorary Life President of the Congresses. If it is doubtfully true, as many maintain, that such congresses serve any great purpose in the furtherance of science, it is unquestionably true that the opportunities they provide for personal contacts are often of very great value. Even if entomologists held differing and sometimes seemingly antagonistic theories, they all sought the hidden truths which the study of differences between species both illuminated and obscured, and the making of friendships could, and often did, provide agreeable opportunities for reconciling them.

Another international field in which Jordan gave great service was that of zoological nomenclature. For about a hundred years from the time of the publication of the 10th edition of Linnaeus's *Systema Naturae*, all seemed well in this rather special field, so great had been the relief afforded by the introduction of the binominal system of naming species. However, by this time, national rivalries and isolation were beginning to cause such confusion that the need for rules of nomenclature was becoming all too evident. Various sets of rules were proposed; for over twenty years argument raged without conclusive result, even within the special Commission which had been set up expressly to produce an acceptable code. By 1913 when the Commission met at Monaco concurrently with the Zoological Congress the situation was critical. It was here that Jordan, quietly but with Lord Rothschild's forthright backing, saved the day by introducing a proposal that the Commission should be given plenary powers to suspend the rules in certain specified cases. This statesmanlike compromise satisfied both opposing camps and has been a major contribution to the stability of zoological nomenclature. It is also a notably successful example of the application of the completely dispassionate and strictly objective scientific approach to the realm of human affairs. It not only 'saved the day' but it has been of the greatest assistance in the settlement of many knotty problems of nomenclature ever since. Jordan remained a member of the Commission till 1950, holding the office of President for just over nineteen years. His farewell letter to the Commission contains the following characteristic passages:

'One or other of the Commissioners may at times be inclined to regret the hours spent on the consideration of nomenclatorial questions which do not directly concern his own field of study; but the thought should ever be present in our minds that nomenclature is an international language and the only international undertaking on the basic principle of which all biologists agree; the same name for the same animal in science throughout the globe. I know that there are highly intelligent biologists who look upon nomenclature with disdain because the great fact has escaped them, that in the present spiritual turmoil in which humanity finds itself one point of general agreement, like the basic principle of nomenclature, renders general agreement in other matters a possibility and gives humanity some hope.'

In the foregoing paragraphs an attempt has been made to indicate the volume of Dr Jordan's contributions to entomology, zoological nomenclature and international co-operation. The full impact of this work and the modernity of his major contributions to the philosophy of evolution and systematics (and their intimate relationships) is only now, after a lapse of more than half a century, being fully appreciated. One reason for this may well be that his most important essays in this field were published either incidentally in the course of systematic monographic treatments of Lepidoptera, or in journals rarely consulted by the general zoologist. His 1896 paper on mechanical selection and other problems appeared in the Tring Museum journal, *Novitates Zoologicae*, which consisted almost entirely of systematic revisions and descriptions; his 1905 paper on the difference between geographical and non-geographical variation appeared in German in the *Zeitschrift fur Wissenschaftliche Zoologie* and is very little referred to in the English literature. Moreover, he worked with insects, which like his methods were not in favour amongst the laboratory biologists of his time. Another and more personal reason was, in the writer's opinion, due to an element of shyness: he was not the type to stump the country in support of any theory or to engage in polemics. Sure of his own position and equally sure that it was right, he seemed always content to let others argue among themselves, in the certain belief that his view could not fail in the end to win acceptance. If this sounds like conceit, it was humble conceit nevertheless. As Lord Rothschild remarked: 'The fellow is always right!' His single-minded purpose was the pursuit of truth; personal prestige interested him not at all.

The whole of Jordan's vast output was consistently directed towards well-defined and easily recognizable objectives, the elucidation of the course and nature of evolution through the most thoroughgoing examination of existing species from every aspect available to him. In the process he set himself to demonstrate that taxonomy, far from being the Cinderella of the biological sciences, held the key to the understanding of evolution and, in its truest expression, was a synthesis of many biological disciplines. 'Jordan had learned as a zoology student at the university that the taxonomist has a great deal of information of vital significance to the general biologist . . . for long it seemed like a hopeless endeavour to bring this point home, but the increasingly close collaboration between taxonomists and experimental biologists during the past two decades proves that the point has finally been made—Jordan has contributed his share toward the victory of the idea that every taxonomist must have a broad training in general biology, and that every biologist must know and understand the important generalizations that come from the field of systematics.' (Mayr, 1955, p. 47.) In championing this cause it was against the superior beings who, *de haut en bas*, looked down on taxonomists as a lower order of zoologists that he inveighed; criticism of his less gifted colleagues was always kindly. But he was careful to point out that the theorist may be all too easily led astray by bad systematics, as Darwin himself was in the case of the birds of Madeira and the Canary Islands.

Jordan's longer papers on systematics have already been mentioned above. One of his most perfect gems, however, is quite a short paper modestly and inadequately entitled 'Notes on Arctiidae' (*Nov. Zool.* **23,** 124-150). In the compass of some 26 pages, Jordan provides a masterly example both of straightforward taxonomy and of the direct application of the resulting conclusions to the much wider field of evolutionary thought. He concludes, in fact, with one of the earliest and clearest statements on the subjects of synpatric and allopatric speciation: ' . . . I have dealt with insects corresponding to two species of the *Lep. Phal.* of Hampson, *Ammalo insulata* and *Sychesia dryas* (as *Elysius dryas* in Hampson). We have seen that under the garb of *insulata* three species are concealed, and that there are nine species of *Sychesia* instead of one (Hampson, *dryas*) or three (Rothschild, *dryas*, *pseudodryas* and *omissus*). The conclusion to be drawn from the results of our investigation is obviously this: In order to arrive, without evidence from breeding, at fairly accurate systematics it is insufficient to study only the external aspect of the specimens . . .

'The differences between the species above described being essentially such as are not visible outwardly, the specific distinguishing characters cannot have developed by means of selection on the part of insectivorous enemies.

'In some of the species we find small but obvious morphological differences in the specimens from certain localities: geographical varieties or *subspecies*. In other forms which are also geographically separated the differences are so great that we must consider them specific; these species replace one another, one being a substitute for the other: *vicariant species*. And lastly, we have species the ranges of which overlap or are more or less the same, the species occurring side by side, often actually flying together: *synpatric species*. This graduation in the evolution is represented, for instance, by (1) *Sychesia dryas dryas* and *S. dryas tupus*, which exclude one another geographically, but are essentially the same insect; (2) *Ammalo insulata*, *A. arravaca* and *A. aurata*, which also inhabit separate geographical areas, but are so different that they might occur together without mixing and amalgamating; and (3) *Sychesia dryas*, *S. subtilis* and *S. omissus*, which occur together. If the differences in the first category become greater, we have the second. If the range of the second category extends, we have the third category of forms. This is true not only for insects, but also for other classes of animals, the exceptions appearing to me more seeming than real. The important part which geographical isolation plays in the evolution of the subspecies and vicariant species is so obvious that it is hardly necessary to dilate on it. Which, however, is the factor or group of factors that led to the appearance of the structural differences we have described? Mendelism cannot account for the geographical phenomenon embodied in the problem; selection by insectivorous enemies being likewise excluded, there remains the influence of the inorganic surroundings, which are different in the various geographical areas, in connexion with geographical isolation. If the geology of a continent or archipelago is known, i.e. the relative ages of the districts or islands, one can generally predict with a high degree of accuracy where subspecies and vicariant species will be found.

'I have assumed that the various species of *Sychesia*, and the three yellow *Ammalo*, have retained the colouring of the respective ancestors from which they are derived, and for this reason are externally so similar to one another. The opinion, however, might be advanced that these species were originally also different in colouring, and their present synchromatism is a secondary development due to mimicry. This cannot be a true explanation, because (1) most of the species are not synpatric, and (2) the subspecies of *S. dryas* and *S. subtilis* are alike in colour and different in structures in a similar way as are the species, but to a much lesser degree.'

Most of Jordan's major contributions to the elucidation of the problem of the factors of evolution, particularly speciation and micro-evolution, were made at a time when the role of the environment was little understood and nothing was known of the nature of genetic factors. It is remarkable, therefore, that in his early discussions of the role of natural selection in the production of mimicry in butterflies he arrived at a theory to account for mimetic polymorphism which has since proved acceptable to geneticists. In order to account for the well-known rarity of mimics, as compared with their models, 'he advanced a theory which—anticipating the recent selection experiments of Mather, Lerner and others—has a remarkably modern ring . . . almost fifty years have passed by before the analysis of gene complexes by Mather and others has led to a genetic confirmation of

Jordan's views of the detrimental effects of one-sided selection.' (Mayr, p. 64.)

When the writer of this brief and inadequate tribute to the memory of Dr Jordan became interested in the systematics of the Rhopalocera, some fifty years ago, Jordan was already pre-eminent and indeed the acknowledged leader. His methods, his approach, and his belief in the importance of the role to be played by true systematics were already widely accepted standards. They were part of one's creed, as it were; indeed, they were largely taken for granted, and the extent to which they were due to the writings of Jordan, and to one's contacts with him was not then fully appreciated. Only now, looking back, does one realize what a debt is due to him. As Mayr has written, 'in his writings and through his contacts he exerted an influence on the development of systematics and on evolutionary thought that cannot be overestimated. Even though Jordan's name may not be cited as widely in textbooks of evolution and zoological systematics as it deserves, at least . . . most of the concepts which he pioneered and for which he fought have now been accepted by virtually every worker in the field'.

For Jordan the man, those who had the boon of his friendship could only feel the warmest affection. At international gatherings he would sweep aside rules, regulations and red-tape with tolerant disdain, coming direct to the thing to be done. Precedents that lay in the past did not concern him; it was only the future that mattered. At work in the Museum, seated at a desk that was to him in perfect order but to anyone else looked like chaos, he was invariably ready to counsel with equal courtesy the novice or the expert: nothing was too much trouble if it served in any way to advance knowledge. To meet him was always a delight, for he was so very human, so unexpectedly humorous, so helpful. At his home in Tring he welcomed entomologists from all parts of the world and the hospitality he dispensed left with all his visitors a sense of having shared something of his contentment in the long life of satisfying and happy achievement that was his.

Karl Jordan died on 12 January 1959.

Grateful acknowledgment is made to the authors of the collection of essays published by the Royal Entomological Society of London on the occasion of Dr Jordan's ninety-fourth birthday (1955, *Trans. R. Ent. Soc. London*, **107,** 1-75). References to these include the author's name. Other references are to the bibliography of Jordan's own published works.

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* Many of the notes in this paper are signed 'K.J.' It is obvious from internal evidence that a long introductory passage on pp. 168-182 is also by him and it is credited to him (not very clearly) by W. Rothschild at the bottom of p. 167. This introductory passage is of importance as Jordan's pioneer essay on methods of classification of Lepidoptera.

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1915. (With W. Rothschild.) Thirteen new Sphingidae. *Novit. zool.* **22,** 281-291, 7 figs. pl. 20, figs. 1-3.

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1915. (With N. C. Rothschild.) On some Siphonaptera collected by W. Rückbeil in East Turkestan. *Ectoparasites,* **1,** 1-24, 27 figs.

1915. (With N. C. Rothschild.) Contribution to our knowledge of American Siphonaptera. *Ectoparasites,* **1,** 45-60, 17 figs.

1916. (With W. Rothschild.) Corrections of and additions to our 'Revision of the Sphingidae'. *Novit. zool.* **23,** 115-123, 3 figs.

1916. A new species of Geometridae from New Guinea. *Novit. zool.* **23,** 123.

1916. Notes on Arctiidae. *Novit. zool.* **23,** 124-150.

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1921. On the replacement of a lost vein in connection with a stridulating organ in a new Agaristid moth from Madagascar, with descriptions of two new genera. *Novit. zool.* **28,** 68-74, 9 figs.

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1921. (With N. C. Rothschild.) A new species of Sarcopsyllidae. *Ectoparasites,* **1,** 131, 132, 2 figs.

1921. (With N. C. Rothschild.) Four new Palaearctic *Ctenophthalmus. Ectoparasites,* **1,** 133-137, 6 figs.

1921. (With N. C. Rothschild.) Two new Palaearctic Siphonaptera. *Ectoparasites,* **1,** 138-140, 3 figs.

1921. Notes on some fleas in the Genoa Museum. *Ectoparasites,* **1,** 141.

1921. (With N. C. Rothschild.) New genera and species of bat-fleas. *Ectoparasites,* **1,** 142-162, 32 figs.

1921. (With N. C. Rothschild.) Eight new *Ceratophylla. Ectoparasites,* **1,** 163-177, 19 figs.

1921. (With N. C. Rothschild.) On *Ceratophyllus fasciatus* and some allied Indian species of fleas. *Ectoparasites,* **1,** 178-198, 30 figs.

1922. On a new genus and species of Polyctenidae from West Africa. *Ectoparasites,* **1,** 199-203, 4 figs.

1922. The American Polyctenidae. *Ectoparasites,* **1,** 204-215, 8 figs.

1922. (With N. C. Rothschild.) New species of Siphonaptera collected by Mr C. Boden Kloss in the Malay Peninsula and south Annam. *Ectoparasites,* **1,** 217-222, 7 figs.

1922. (With N. C. Rothschild.) The Siphonaptera collected by Mr E. Jacobson on Sumatra. *Ectoparasites,* **1,** 223-230, 9 figs.

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1922. Notes on the distribution of the organ of Berlese in Clinocoridae. *Ectoparasites,* **1,** 284-286, 5 figs.

1922. On an organ peculiar to the females of some genera of Ludiinae, a subfamily of Saturniidae. *Novit. zool.* **29,** 247-248, 4 figs.

1922. A monograph of the Saturnian subfamily Ludiinae. *Novit. zool.* **29,** 249-326, 169 figs. pls. 1, 2.

1922. Die von G. Tessmann in Spanisch Guinea gesammelten Anthribiden, nebst Beschreibungen einiger anderer afrikanischer Arten. (Col.). *Ent. Mitt.* **11,** 131-158.

1922. Einige neue Saturnoideae aus Südamerika. (Lep.). *Ent. Mitt.* **11,** 193-195.

1922. Anthribidae collected by Signor L. Fea on the Islands of Fernando Po, S. Thomé and Principe. *Ann. Mus. Stor. nat. Genova,* **49,** 46-56, 3 figs.

1922. Stridulating organs in *Holacera,* etc. *Proc. Bnt. Soc. Lond.* **1921,** ii-iv.

1922. A new species of *Exillis,* a genus of Anthribidae (Col.). *Entomologist,* **55,** 152.

1923. Les Anthribides de l'Indochine. *Faune ent. Indochine,* **6,** 71-113 (and *Opusc. Inst. sci. Indochine* no. 1, 41, pp.).

1923. (With N. C. Rothschild.) Additions to the catalogue of Swiss Siphonaptera. *Ectoparasites,* **1,** 287-289, 1 fig.

1923. (With N. C. Rothschild.) Further records of Algerian Siphonaptera. *Ectoparasites,* **1,** 290-292.

1923. (With N. C. Rothschild.) On some Siphonaptera from the eastern hemisphere. *Ectoparasites,* **1,** 293-308, 30 figs.

1923. (With N. C. Rothschild.) New American Siphonaptera. *Ectoparasites,* **1,** 309-319, 16 figs.

1923. (With N. C. Rothschild.) On the genera *Rhopalopsyllus* and *Parapsyllus. Ectoparasites,* **1,** 320-370, 56 figs.

1923. A moth with a stridulatory apparatus. *Proc. Ent. Soc. Lond.* **1922,** xxiv.

1923. The tympanal organ of *Speirodonia* (Noctuidae). *Proc. Ent. Soc. Lond.* **1922,** lxxix.

1923. The scent-organ of certain mimetic Castniidae. *Proc. Ent. Soc. Lond.* **1922,** xci.

1923. On the comb-bearing flap present on the fourth abdominal segment in the males of certain Notodontidae. *Novit. zool.* **30,** 153, 154, pl. 2, figs. 9-13.

1923. On a scent-organ found on the head of many Lepidoptera. *Novit. zool.* **30,** 155-158, pl. 2, figs. 1-8.

1923. On the scent-organs in the males of certain American Castniidae. *Novit. zool.* **30,** 159-162, 7 figs.

1923. A note on the families of moths in which R^2 (= vein 5) of the forewing arises from near the centre or from above the centre of the cell. *Novit. zool.* **30,** 163-166, pl. 3.

1923. New eastern Anthribidae. *Novit. zool.* **30,** 167-185.

1923. Four new Sphingidae discovered by T. R. Bell in North Kanara. *Novit. zool.* **30,** 186-190, 10 figs.
1923. New Anthribidae from the eastern hemisphere. *Novit. zool.* **30,** 216-221.
1923. On *Anaitis efformata* Guen. (1858), a species distinct from *A. plagiata* L. (1758). *Novit. zool.* **30,** 243-246, 9 figs.
1923. A new species of hawkmoth from Borneo. *Novit. zool.* **30,** 268, 269, 2 figs.
1923. List of the butterflies figured on Plate IV. *Novit. zool.* **30,** 270, pl. 4.
1923. Über einige alte und neue Sphingiden (Lep.). *Ent. Mitt.* **12,** 51-57, 10 figs.
1924. New Anthribidae allied to *Choragus*. *Ann. Mag. nat. Hist.* (9), **16,** 606-620.
1924. *Arixenia* associated with bats. *Proc. Ent. Soc. Lond.* **1924,** ii.
1924. A new species of *Thysania* allied to *T. agrippina*. *Proc. Ent. Soc. Lond.* **1924,** xvi, xvii.
1924. A race of *Asota heliconia* L. (Hypsidae), which has lost the orange warning colour of the body. *Proc. Ent. Soc. Lond.* **1924,** xxxiv, xxv.
1924. On the Saturnioidean families Oxyctenidae and Cercophanidae. *Novit. zool.* **31,** 135-193, pls. 6-21.
1924. On the West African Lymantriid genus *Nyctemera* (=*Otroeda* (Lep. Het.)). *Novit. zool.* **31,** 217-226, 18 figs.
1924. Anthribidae from the island of Rodriguez. *Novit. zool.* **31,** 227-230.
1924. New Anthribidae. *Novit. zool.* **31,** 231-255.
1924. On some genera of Anthribidae allied to *Exillis* Pasc. (1860). *Novit. zool.* **31,** 256-260.
1924. On *Hypocista* and some allied genera of Satyrinae (Lep. Rhop.) from New Guinea and the Solomon Islands. *Novit. zool.* **31,** 279-297.
1924. Two new subspecies of Sphingidae. *Novit. zool.* **31,** 298.
1925. Siphonaptera. In Th. Monod. Parasitologia Mauritanica, materiaux pour la faune parasitologique en Mauritanie. *Bull. Com. A.O.F.* **1925,** 101, 102.
1925. New Anthribidae collected by J. C. Corporaal on Sumatra. *Ann. Mag. nat. Hist.* (9), **16,** 257-264.
1925. On the position of *Urodon*, a genus of Coleoptera. *Proc. Ent. Soc. Lond.* **1924,** xcviii, xcix.
1925. New Siphonaptera. *Novit. zool.* **32,** 96-112, 46 figs.
1925. Siphonaptera collected by Rear-Admiral H. Lynes in Darfur in 1920 to 1922. *Novit. zool.* **32,** 113.
1925. New Oriental Zygaenidae. *Novit. zool.* **32,** 230-237.
1925. A new species of Anthribidae from Sumatra collected by E. Jacobson. *Novit. zool.* **32,** 238.
1925. Five new Indian Anthribidae. *Novit. zool.* **32,** 239-241.
1925. Anthribidae from the eastern hemisphere. *Novit. zool.* **32,** 242-257.
1925. On *Delias belladonna* and allied species (Lep. Rhop.). *Novit. zool.* **32,** 277-287, 8 figs.
1925. On some subspecies of African *Charaxes*. *Novit. zool.* **32,** 288, 289.
1925. Two new Indian Anthribidae, received from the Forest Research Institute and College at Dehra Dun. *Novit. zool.* **32,** 290, 291.
1926. Über die Stellung der Tagfaltergattung *Libythea*. *Int. Congr. Ent.* III (Zurich, 1925), **2,** 361-366, 7 figs.
1926. On *Xenopsylla* and allied genera of Siphonaptera. *Int. Congr. Ent.* III (Zurich, 1925), **2,** 593-627, 16 figs. pls. 17-20.
1926. Some new eastern Sphingidae in the Tring Museum. *Ann. Mag. nat. Hist.* (9), **18,** 207-209.
1926. Coleoptera. Anthribidae from Juan Fernandez. *Nat. Hist. Juan Fernandez* and *Easter Island,* **3,** 479, 480.
1926. (With N. C. ROTHSCHILD.) Siphonaptera from Borneo. *Sarawak Mus. J.* **3,** 287-292, 7 figs.
1926. Anthribidae from northern Sarawak. *Sarawak Mus. J.* **3,** 371-373.
1926. Some new Anthribidae in the British Museum. *Novit. zool.* **33,** 146-154.
1926. New eastern Anthribidae in the Tring Museum. *Novit. zool.* **33,** 155-170.

1926. The Agaristidae and Zygaenidae from the Bismarck Archipelago in the Tring Museum. *Novit. zool.* **33,** 358-366.

1926. On a Pyralid parasitic as larva on spiny Saturnian caterpillars at Para. *Novit. zool.* **33,** 367-370.

1926. Some new Agaristidae, with remarks on nomenclature. *Novit. zool.* **33,** 371-378, 1 fig.

1926. On some Old World Sphingidae. *Novit. zool.* **33,** 379-384, 7 figs.

1926. New Siphonaptera. *Novit. zool.* **33,** 385-394, 22 figs.

1927. A teratological specimen of *Melanitis leda* from Formosa. *Proc. Ent. Soc. Lond.* **2,** 20, 2 figs.

1928. [Scent-organ in male of *Arsenura.*] *Proc. Ent. Soc. Lond.* **2,** 74.

1928. Anthribidae. *Insects of Samoa.* Part 4 fasc. **2,** 161-172, 11 figs.

1928. Further records of Anthribidae from French Indo-China, with the addition of the descriptions of two new species from other countries. *Novit. zool.* **34,** 77-94.

1928. Anthribidae from the Malay Peninsula. *Novit. zool.* **34,** 95-104.

1928. New Anthribidae from the Old World. *Novit. zool.* **34,** 105-128.

1928. On some Lepidoptera of special interest, with remarks on morphology and nomenclature. *Novit. zool.* **34,** 132-146, pls. 1-3.

1928. On plates IV and V, representing Oriental Epiplemidae. *Novit. zool.* **34,** 147-150, pls. 4, 5.

1928. Some Anthribidae collected by R. E. Turner in South Africa. *Novit. zool.* **34,** 151-158.

1928. On the *latreillei*-group of eastern Papilios. *Novit. zool.* **34,** 159-172, pls. 6, 7.

1928. Siphonaptera collected in the Dolomites. *Novit. zool.* **34,** 173-177, 6 figs.

1928. Siphonaptera collected during a visit to the eastern United States of North America in 1927. *Novit. zool.* **34,** 178-188, 4 figs.

1929. Exhibition of some Sphingidae. *Proc. Ent. Soc. Lond.* **3,** 77.

1929. South American parasitic fly larvae. *Proc. Ent. Soc. Lond.* **3,** 78.

1929. Butterflies from the Herzog Mts. in eastern New Guinea. *Proc. Ent. Soc. Lond.* **4,** 12, 13.

1929. On a Brazilian sand-flea, *Tunga caecata* Enderl. 1901. *Proc. Ent. Soc. Lond.* **4,** 34, 35.

1929. On the difference in position of certain ♂-characters in some allied genera of Anthribidae. *Proc. Ent. Soc. Lond.* **4,** 50-52, 6 figs.

1929. Notes on North American fleas. *Novit. zool.* **35,** 28-39, pls. 1, 2.

1929. A new Anthribid (Coleopt.) from Italian Somaliland. *Bull. Soc. ent. ital.* **61,** 165, 166.

1929. Fauna Buruana. Coleoptera, Fam. Anthribidae. *Treubia,* **7,** 331-340, pl. 6.

1929. On some problems of distribution, variability and variation in North American Siphonaptera. *Int. Congr. Ent.* IV (Ithaca, N.Y. 1928), pp. 489-499, 11 figs.

1929. Some Old-World Siphonaptera. *Novit. zool.* **35,** 40, 41, 3 figs.

1929. On *Oxyambulyx substrigilis* and some allied Sphingidae. *Novit. zool.* **35,** 60-62, pl. 3.

1929. On some Oriental Sphingidae. *Novit. zool.* **35,** 85-88, 5 figs.

1929. Further records of North-American bird-fleas, with a list of the Nearctic birds from which fleas are known. *Novit. zool.* **35,** 89-92.

1929. On fleas collected by Dr H. M. Jettmar in Mongolia and Manchuria in 1927 and 1928. *Novit. zool.* **35,** 155-164, pls. 7-10.

1929. Two new African species of *Ctenophthalmus* (Siphonaptera). *Novit. zool.* **35,** 165-167, 4 figs.

1929. On a small collection of Siphonaptera from the Adirondacks, with a list of the species known from the state of New York. *Novit. zool.* **35,** 168-177, 4 figs.

1929. Some new Palaearctic fleas. *Novit. zool.* **35,** 178-186, 10 figs.

1929. On *Polyptychus pygarga* and some allied species (Lep. Sphingidae). *Novit. zool.* **35,** 187-191, 12 figs.

1930. Two new American fleas. *Novit. zool.* **35,** 268, 269, 2 figs.

1930. Some new butterflies and moths from eastern New Guinea. *Novit. zool.* **35,** 277-287, 6 figs.

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1930. On some South African fleas. *Novit. zool.* **36,** 129-138, 7 figs.
1930. Three new Anthribidae from British India. *Novit. zool.* **36,** 143-145.
1930. On the two species of *Deudorix* known from New Guinea (Lep., Lycaenidae). *Novit. zool.* **36,** 147-149.
1931. The President's address. *Proc. Ent. Soc. Lond.* **5,** 128-142, 11 figs.
1931. Three new species of *Neopsylla* (Siphonaptera) from the Oriental Region. *Novit. zool.* **36,** 220-224, 5 figs.
1931. Siphonaptera collected by Mr F. J. Cox in France. *Novit. zool.* **36,** 225-229, 4 figs.
1931. Records of fleas from the Austrian Tirol and the Dolomites. *Novit. zool.* **36,** 230-232, 1 fig.
1931. On some fleas collected by Monsieur Heim de Balzac in western Algeria. *Novit. zool.* **36,** 233, 234, 4 figs.
1931. On *Cypa decolor* and some allied species (Lep., Sphingidae). *Novit. zool.* **36,** 235-242, 24 figs.
1931. On the geographical variation of the pine hawk-moth, *Hyloicus pinastri. Novit. zool.* **36,** 243-249, 12 figs.
1931. Two new Zygaenidae (Lepid.). *Novit. zool.* **36,** 277, 278, 4 figs.
1931. Anthribidae versus Patystomidae. *Novit. zool.* **36,** 281-287.
1931. Anthribidae collected by F. C. Drescher on the island of Java. *Novit. zool.* **36,** 288-302, 4 figs.
1931. Two new Oriental Anthribidae. *Novit. zool.* **36,** 303, 304.
1931. Some Anthribidae from tropical Africa. *Novit. zool.* **36,** 305-310, 1 fig.
1931. Three new South American fleas. *Novit. zool.* **36,** 311-316, 5 figs.
1931. Both sexes of *Papilio aristor*, God., from San Domingo. *Proc. Ent. Soc. Lond.* **6,** 62.
1931. Flöhe aus Venezuela. *Z. Parasitenk.* **3,** 264-266, 1 fig.
1931. Further records and descriptions of fleas from Ecuador. *Novit. zool.* **37,** 135-143, 12 figs.
1931. Three new Old World fleas. *Novit. zool.* **37,** 144-147, 6 figs.
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1932. The Siphonaptera collected by Mr J. L. C. Musters in Norway on the lemming. *Novit. zool.* **38,** 256, 257, 2 figs.
1932. Siphonaptera collected by Mr C. Elton in Lapland. *Novit. zool.* **38,** 258-260, 1 fig.
1932. Siphonaptera collected by Herr Georg Stein in the High Tatra. *Novit. zool.* **38,** 261-263, 4 figs.
1932. A new *Xenopsylla* from Hawaii. *Novit. zool.* **38,** 264-266, 4 figs.
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1932. Some new African Anthribidae. *Novit. zool.* **38,** 295-300.
1932. Further records of Anthribidae from Java. *Novit. zool.* **38,** 301-304.
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1933. Siphonaptera collected by Mr F. Shaw Mayer in Mandated New Guinea. *Novit. zool.* **39,** 55-61, 8 figs.
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1933. Descriptions of Siphonaptera. *Novit. zool.* **39,** 66-69, 7 figs.
1933. A survey of the classification of the American species of *Ceratophyllus s. lat. Novit. zool.* **39,** 70-79.
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1937. On some North American Siphonaptera. *Novit. zool.* **40,** 262-271, 13 figs.
1937. A further collection of Siphonaptera obtained by Mr F. Shaw Mayer in eastern New Guinea. *Novit. zool.* **40,** 272-282, 13 figs.

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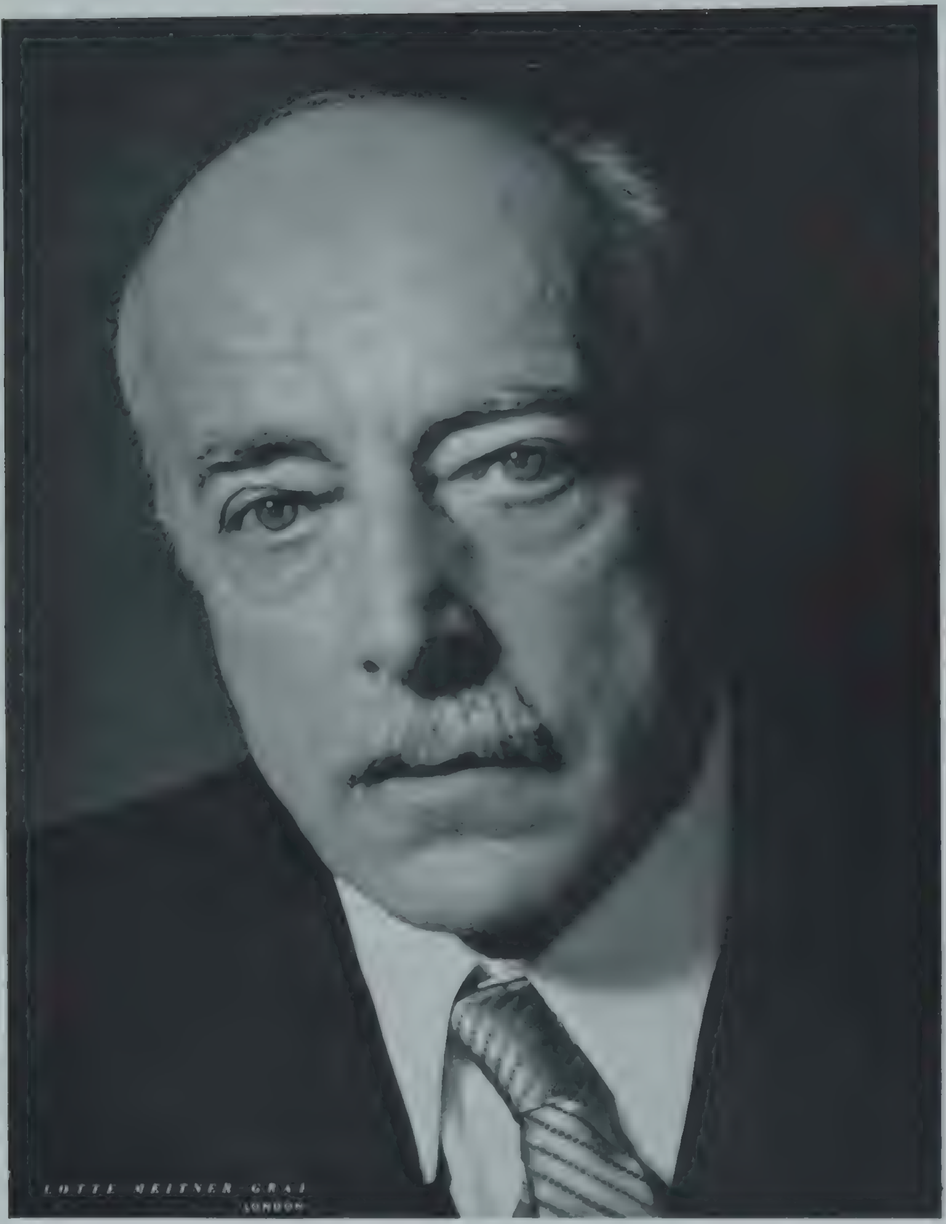
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M. v. Laue

MAX von LAUE

1879-1960

In Max von Laue the world has lost one of the great physicists of the period of transition from classical physics to quantum physics. His work can be roughly divided by subject matter into thermodynamics of radiation, optics, including X-ray optics, relativity, and superconductivity. Best known is, of course, Laue's discovery of the diffraction of X-rays by crystals in 1912, which was carried out experimentally by his collaborators W. Friedrich and P. Knipping. So evident was the importance of this work for elucidating the —up to then only surmised—nature of both X-rays and crystals that the 1914 Nobel Prize for Physics was awarded to Laue—at that time a very prompt recognition of a fundamental investigation in physics. Two great branches of the physico-chemical sciences grew out of Laue's discovery: X-ray spectroscopy which gave invaluable help in the development of atomic theory, and X-ray crystal structure analysis, alias X-ray crystallography, which revolutionized the scope and precision of our understanding the solid state, mainly from a chemical point of view. In neither of these two scions of his original idea did Laue take more than a receptive interest, whereas the more physical part of X-ray optics, later enlarged by electron optics, focused his creative work up to the last weeks of his life.

Max (Theodor Felix) Laue was born on 9 October 1879 in Pfaffendorf near Koblenz as the son of a civil official of the German military administration.* Owing to various shifts in his father's assignments he visited classical schools (Gymnasien) in Posen, Berlin and Strassburg. It was by a chance remark on the electrolytic deposition of copper from a solution of copper sulphate which he heard at the age of 12 or 13 that his interest in physics arose. Laue himself writes†: 'The impression made upon me by this first encounter with physics was enormous. For several days I went about in deep thought and was good for nothing, so that my mother enquired anxiously what was wrong with me. After having found out, she saw to it that I went to the "Urania" frequently. This was a society for popularizing science in whose premises in the Taubenstrasse in Berlin a great display of physical apparatus was to be found with full explanations and ready to be set in action by pressing a button.' Later, Laue found an inspiring teacher of mathematics and physics at the gymnasium in Strassburg and read and re-read H. v. Helmholtz's just published *Vorträge und Reden*, including the more philosophical ones.

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* The father was raised to hereditary nobility in 1913; from there on Laue signs M. v. Laue.

† See his autobiographical essay (Bibliography, 1952).

These early impressions left a deep mark on Laue; his numerous occasional speeches, messages and obituaries contain ample proof of his unusual familiarity with the German classical authors and poets as well as with the general writings of the great physicists.

Laue began studying mathematics, physics, and chemistry during the winter 1898-99 in Strassburg while he was serving his compulsory year of military training. Ferdinand Braun's elegant demonstrations in the introductory physics course increased his enthusiasm for physics still further. In the following year, studying in Göttingen, Laue found out, under the combined influence of Woldemar Voigt's lecture course and the study of G. Kirchhoff's published lectures, that his own calling was theoretical physics. After two years in Göttingen and a further winter semester in Munich, Laue settled in Berlin in 1902 in order to obtain a degree. Here he encountered Max Planck and soon began work on a thesis concerning the application of the principle of conservation of energy in the case of interference phenomena with plane-parallel plates. He received the degree of Dr.phil. in July 1903 'magna cum laude', with mathematics and philosophy as minors. But he was not yet satisfied with his knowledge of the art and added two years of post-doctoral studies in Göttingen, attracted by the small town and the excellence of the senior and junior staff at this university. Here Laue also took the examination required for high-school teaching (Lehramtsexamen) in the course of which he had to answer questions in mineralogy. To the increasing merriment of his examiner, Professor Koenen, Laue confessed more and more his ignorance of the subject—but he was passed by compensating this with his knowledge of chemistry which was above average. Laue himself suggests that, maybe, this very unfamiliarity had the happy effect of letting him approach much later (1912) a problem of crystal physics in a perfectly unbiased way. Only after his great discovery, while Professor in Frankfurt, in 1914-18, did Laue attempt to fill in the gap, acquiring, if not proficiency in crystallography, at least a strong liking for crystals.

In the autumn of 1905 Planck offered Laue the position as assistant at the Institute for Theoretical Physics and this brought Laue back to Berlin and into close personal contact with Planck from 1905 to 1909. Two main results of this period are outstanding. The first concerns the application of the entropy concept to radiation fields, and the thermodynamical significance of the coherence of light waves. A strict definition of the degrees of freedom connected with radiation resulted from these considerations. The second direction of Laue's research in Berlin, and later, goes back to a meeting of the Berlin Physics Colloquium in which Planck reported on the recently published paper by A. Einstein (September 1905) on the electrodynamics of moving bodies. Laue absorbed this paper with the utmost intensity and overcame the many objections which the novel concepts of space and time did not fail to raise in his mind, as in that of most other physicists. By 1907 he had fully accepted Einstein's theory and the first of a series of papers discussing the applications of the (special) theory of relativity appeared, namely, the derivation of Fresnel's coefficient of drag for light passing through moving media. Seven more papers on relativity appeared in the next four years.

Laue started his academic career soon after having returned to Berlin by becoming lecturer (Privatdozent) in 1906. In 1909, however, he decided to change over to the University of Munich where A. Sommerfeld was then building up a strong school of theoretical physics. At first Laue continued to work on relativity, writing, on the suggestion of the publishers Vieweg & Sohn, the first monograph on this subject. This book of some 200 pages, first called *Das Relativitätsprinzip*, and in later editions *Die Relativitätstheorie*, Bd. 1, was from the first edition (1911) and still is in the sixth edition (1955) an unfailing source of exact information on the physical aspect of the special theory of relativity. It exemplifies the thoroughness and precision of Laue's work which is characteristic for his production throughout.

Laue's lectures in Munich were mainly on thermodynamics, optics, theory of radiation and relativity. Since the main series of courses in theoretical physics was given by the Professor, Sommerfeld, Laue, as well as the other lecturer, P. Debye, gave short courses on rather specialized subjects. Laue, unfortunately, was far from being an orator; his speech was of a hasty and uneven flow and quite often hard to understand. His writing on the blackboard was only little better, so that all but the most determined students dropped out. But those who endured were rewarded by the grandeur, enthusiasm and impeccable honesty of the ideas behind the unattractive presentation of the course.

Sommerfeld was the editor in charge of volume 5 of the monumental *Enzyklopädie der Mathematischen Wissenschaften*, a favourite scheme of his great teacher and friend, Felix Klein, in Göttingen, which had as its aim a review of pure and applied mathematics in the early years of the twentieth century. Volume 5 was assigned to theoretical physics and Sommerfeld asked Laue to write the chapter on wave optics. In this Laue was engaged at the time when he conceived the idea of X-ray diffraction in crystals in February 1912. Laue himself, in his Nobel Lecture, tells how, on discussing problems concerning the passage of light waves through a periodic, crystalline arrangement of particles, the idea came to him that much shorter electromagnetic waves, such as X-rays were supposed to be, must give rise to some kind of interference or diffraction phenomenon in such a medium. 'My optical feeling told me this' Laue writes—and indeed, the conscious application of the optical principles to the formulation of the conditions for obtaining diffracted X-rays came much later, namely, as soon as the first successful experiments had proved the existence of the effect.

Between the first conception and the realization of the experiment some difficulties had to be overcome. Sommerfeld, W. Wien, Mie and other eminent physicists who used to gather in the alps for skiing in the first half of the Easter vacations discussed the proposed experiment and ended up by considering it hopeless because of the disturbing influence of the temperature motion of the atoms. The same doubts were expressed in the group of young physicists who used to gather in the Café Lutz in the Hofgarten in Munich after their midday meal. Here, however, the tendency won out that the experiment might as well be tried, and W. Friedrich, Sommerfeld's recently appointed experimental assistant, was ready to undertake it. This did not find the approval of Sommerfeld who was keen on Friedrich's checking experimentally some theoretical predictions of Sommerfeld's concerning the angular distribution of X-rays coming from the anti-cathode. However, Friedrich and another doctorand of Röntgen who had just graduated, P. Knipping, set up a first, quite primitive arrangement and went ahead with the first exposure. They used a copper sulphate crystal, firstly because it was at hand, and secondly because it was known to them that the tube they were using strongly excited the copper characteristic radiation, and this they considered to indicate a strong interaction. Friedrich estimated the necessary time of exposure to be about 10 hours, judging by the exposures Herwegh had used in his experiments on polarization of X-rays by secondary scattering on graphite. The commercial, medical, gas-filled X-ray tube had a platinum target and it was fed by an induction coil. The first exposure gave no result. The photographic plate had been placed between the crystal and the tube, in expectation of an effect similar to that of a reflexion grating. On Knipping's insistence a second exposure was made placing plates on the exit side, and to the sides, of the crystal. The plate on the exit side showed strong, rather blurred spots surrounding the trace of the direct beam. Laue first heard of this result when he came to Café Lutz and rushed back to the Institute to see the result. He then walked home and on the way the mathematical formulation of the effect appeared to him—namely, the three-fold application of the simple condition of diffraction by a linear grating.

After this first result, Sommerfeld helped the further investigation of the effect in every way. A more elaborate apparatus was constructed in which an accurately oriented crystal could be exposed to a well-collimated X-ray beam; crystal plates of suitable thicknesses were ordered from Steeg & Reuter and, later, a medical X-ray firm, Reiniger, Gebbert & Schall lent or donated a much more powerful transformer and spark converter (Ideal Apparat). Röntgen came to see the experiment and the plates as soon as he heard of them; he was profoundly interested and had to agree that the phenomenon was caused by the crystal structure (since on exposing the pulverized crystal the spots did not appear) but he declined to attribute the spots to diffraction. (Röntgen, incidentally, had searched for diffraction of X-rays as reported in his famous original papers of 1896 and 1897, and he had also sent X-rays through a single crystal of rock-salt and the same amount of pulverized rock-salt, measuring in both cases the extinction of the X-ray, in an attempt to determine the refractive index for X-rays from the Christiansen filter effect. What a chance he had of making Laue's discovery and of establishing the nature of his X-rays had he but possessed more powerful tubes!)

In order clearly to establish that they were dealing with the expected diffraction effect, Laue, Friedrich and Knipping used two zincblende plates, one normal to a fourfold axis of symmetry, the other normal to a threefold axis, and well adjusted on the improved apparatus. The photographs, unsurpassed for clarity and precision by any later Laue diagrams, eliminated all doubt, especially when Laue applied to them the mathematical formulation of the conditions of diffraction and showed that each spot could be accounted for as the diffraction of a certain order, described by three integers.

The publication of the discovery took place in the Bavarian Academy of Sciences; the first paper, containing the general theory, including the three Laue equations, and the experimental part with the photographs was presented by Sommerfeld (as a member of the Academy) on 8 June 1912, and the second part, containing Laue's detailed discussion of the diagrams, on 6 July 1912. Although, in those days, the daily press ignored scientific discoveries, the news of these fundamental results spread quickly, so that, for instance, at the British Association Meeting in Dundee, 4 to 11 September 1912, Lord Rayleigh commented on Laue's discovery, as did also Sir Oliver Lodge in the Becquerel Memorial Lecture on 17 October 1912. A. E. H. Tutton, who had visited P. v. Groth in the course of the summer gave an account of Laue's discovery in *Nature*, 14 November 1912, but even before that the first of a series of letters to *Nature* by W. H. Bragg appeared on 24 October 1912. This was probably the result of the reprints Laue sent out to those known to be working on X-rays.

Looking back, it is striking to note the natural division in the interests of those who followed up Laue's discovery. In England, W. H. and W. L. Bragg developed the methods of crystal structure analysis. Their great advantage was W. H. Bragg's familiarity with characteristic radiation, and W. L. Bragg's happy concept of considering reflexion on a set of atomic planes rather than diffraction by a three-dimensional grating as the unit of explanation. Together, these two ideas led to the construction of the X-ray spectrometer by means of which plane after plane of a crystal structure could be investigated and measured out in terms of known wavelengths. In the hands of the two eminent physicists endowed with an unusually powerful three dimensional vision this instrument served to unravel one crystal structure after the other and to build up a school of scientists who, whether they came from physics or chemistry, were mainly interested in deciphering the atomic arrangement in crystals and thereby to delve deeper than ever before into the mysteries of chemical interaction. The X-ray spectroscopists, M. de Broglie, M. Siegbahn, Coster, and others, developed the instrumentation and methods for measuring X-ray wavelengths with ever-increasing precision and resolution.

Laue, on the other hand, was fully satisfied when he saw that his physical idea worked. In the summer of 1912 he accepted a chair of theoretical physics at the University of Zürich—but even before he left Munich he had set up an experiment on the diffraction of light by a dust-covered glass plate which had nothing to do with X-ray diffraction. He wrote on one occasion*:

'Once I had conceived the idea of X-ray diffraction in crystals this appeared so evident to me that I never understood the astonishment it produced among scientists, nor, indeed, the doubts expressed in the course of the first few years. That Bragg's idea of reflexion on atomic planes was essentially equivalent to mine—this I saw at once, and I published it early in 1913. Now access to short electromagnetic waves had been found. This extension of the optical spectrum is the counterpart of H. Hertz's discovery of the long electromagnetic waves.'

Before dealing with Laue's further scientific work, let us survey his career. He held the chair in Zürich for two years and was then called to Frankfurt/Main, where he was during the war, 1914-18. Part of this time he worked with a group of physicists headed by W. Wien, in the Physics Department of the University of Würzburg on the physics of electronic tubes. Important papers on the thermodynamics of an electron gas resulted from this work.

With the end of the war, Laue became Professor of Theoretical Physics at the University in Berlin; he held this position from 1919 to 1943. It was a position created for him which did not burden him with much course work. His influence made itself felt mainly in specialized courses and seminars and other graduate work. Besides, he was consultant at the Physikalisch-Technische Reichsanstalt in Berlin-Charlottenburg. Here, he took an active part in the discussion of Walther Meissner's work on superconductivity and, in particular, explained in 1932 the fact that the threshold of the applied magnetic field, destroying superconductivity, varies with the shape of the body because of the magnetic field deformation by the super-currents induced at the surface of the metal in the case where the magnetic field is established after the superconducting state has been reached. 'This explanation', Laue claims (*loc. cit.*), 'led W. Meissner to the important discovery that a magnetic field established in the metal at higher temperature is expelled from it as it becomes superconducting—an effect not explainable by Maxwell's theory.' Actually, however, Meissner was led to his statement from the observation that a volume current flowing in a metal above the transition point condenses to a surface current when the metal becomes superconducting. The elimination of magnetic flux from the interior of a superconductor became the basic idea of F. and H. London's theory of superconductivity. One point of this theory, violating the conservation of energy, was discussed in a joint paper by Laue and the Londons (1935). Laue also wrote important papers on the distribution of current in branched superconductors. The results of twelve papers by Laue on various aspects of superconductivity, published from 1937 to 1947, were incorporated in the first (1947) and second (1949) editions of his book *Theorie der Supraleitung*, which attempts a purely phenomenological approach to the subject.

As early as 1920 the high distinction was conferred on Laue of being made Member of the Prussian Academy and with it began an increasingly important part of Laue's work, namely, his influence on the development of German science and scientific institutions. Laue took this task very seriously, and beside the great amount of positive promotion of scientific projects channelled through him, there is perhaps an equivalent amount of damage prevented by his vigilance and resoluteness.

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* MS. Persönliches über meine Arbeiten (unpublished).

Another close link which lasted up to his death was formed in 1917 when the Kaiser Wilhelm Institut für Physik in Berlin-Dahlem was founded. Albert Einstein was its Director, but on Laue, the second Director, fell most of the administrative work. The Institute had no building but—more important—it had a budget, by means of which it supported scientific research in Germany. During the period of inflation the capital behind this budget shrank to insignificance and the parliament-supported organization, the Notgemeinschaft der Deutschen Wissenschaft, took over the support of scientific activities. In an endeavour to re-activate the K.W.I. for Physics, the Rockefeller Foundation invited von Laue and Ladenburg in 1930 to visit physics institutes in the U.S.A. with a view to planning the German Institute. The report they delivered led to the decision of establishing this institute with J. Franck as director of the experimental part and Laue director of the theoretical section. Under the Nazi policy Franck left Germany in 1933 and P. Debye was installed in 1936 as the Director of the Institute, which had been erected in the meanwhile, Laue being the head of a semi-independent section in which Menzer, Borrmann, Molière, Lamla and others were engaged on theoretical and experimental work along the lines of Laue's interests.

This state of affairs was not to last long. Late in 1938 Otto Hahn and F. Strassmann working in the adjoining K.W.I. for Chemistry, found nuclear fission and in view of the importance of this discovery for the production of energy, the K.W.I. for Physics was taken over by the Army. This change prompted Debye, as of Dutch nationality, to relinquish his position and to leave Germany. Laue refused to take over responsibility for the nuclear work and retained only his own section. By 1942 the 'Uranium Project' was severed from the Army and the Institute was returned to the Kaiser Wilhelm Gesellschaft which appointed W. Heisenberg as director. Owing to the bombing of Berlin the Institute was evacuated in 1944 to a small town in Württemberg, Hechingen, and Laue who, although retired prematurely by the Minister of Education on 1 October 1943, had remained in Berlin while the Institute was there, followed to Hechingen. Here he witnessed with a feeling of relief the arrival of French troops, thankful that in spite of all preparations no final defence had ruined the little town. Next day he was taken prisoner by an English-American group carrying out the 'Mission Alsos'. The report on this mission under the direction of Sam Goudsmit is so well known that details can be omitted here. The strict seclusion of the ten scientists in the big mansion Farmhall near Huntingdon in Cambridgeshire ended in January 1946 and Laue returned to Germany. For the next five years he lived in Göttingen under rather restricted circumstances, officially as Acting Director of the Max Planck Institute for Physics and titular Professor at the University. It was a special distinction that Laue was enabled to come to London on his own (though in company with the British Scientific Liaison Officer, Dr R. Fraser) to take part in the first international meeting of crystallographers in London in July 1946 and that he was made the one and only Honorary President of the International Union of Crystallography when this Union had its first Assembly at Harvard University two years later.

In 1951 the Max Planck Gesellschaft, the successor of the Kaiser Wilhelm Gesellschaft, appointed Laue, then in his 72nd year, Director of the Fritz Haber Institute of Physical Chemistry in Berlin-Dahlem. In this position he remained up to his retirement in 1958, taking up to his death a very active part especially in the work on X-ray optics (Borrmann, Hosemann and others) and in the preparation of the third edition of his book *Röntgenstrahlinterferenzen*.

On 9 October 1959, Laue's eightieth birthday was celebrated in Dahlem, and even though this celebration was intended to be restricted to the Institute and the family, congratulations and high honours were heaped on Laue. That two eminent co-octogenarians and long-time close friends and co-scientists, Otto Hahn and Lise Meitner, were among the guests who added to the harmony and serenity of the day.

Laue's death came unheralded and unexpected and although tragic, perhaps was an appropriate end to a life of activity and manliness. On 8 April 1960 Laue was on his way to a committee meeting in close-by Wannsee, driving by himself the Institute's car on one of the speedways, when he collided with a motorcyclist. While the latter (a young man who had obtained his licence only two days previously) was killed outright, Laue's car overturned on the embankment and Laue had to be extracted from under it by the fire brigade. He lay in hospital with heavy concussion and, after a first period of recovery, died on 24 April 1960. His ashes rest next to the remains of Planck, Nernst and other of his peers and friends, in the municipal cemetery in Göttingen.

X-RAY DIFFRACTION

The Fraunhofer treatment of the diffraction of an incident X-ray by the periodic array of atoms in a crystal is given in the original paper of 1912. Three equations expressing full co-operation between all wavelets scattered by the atoms form the foundation for the 'geometrical' theory; they contain all information necessary for devising the various experimental methods of observation, for indexing the diagrams and for determining the shape and size of the unit cell (this being identical with the assignment of indices to the lines or spots on the film). Beyond this, the summation of the scattered spherical wavelets is carried out, and the resulting amplitude obtained, for any directions of observation, not only those of full co-operation. A crystal in the shape of a block of M, N, and P cells, respectively, along the directions of the cell edges is assumed and the 'Laue function', a product of three factors of the form $\sin MA/\sin\frac{1}{2}A$, is found for the resulting amplitude, and its square for the intensity. This expression is the foundation of the 'kinematical' theory, i.e. a theory based on the assumption that the incident X-ray penetrates unencumbered to each atom and that each atom scatters only this wave. In view of the fact that the primary X-ray beam produced a blackening of the photographic plate within seconds, whereas the diffracted spots required hours of exposure time, the assumption of the preponderance of the primary beam appeared justified. Applied to a crystal structure with more than one atom per cell, the kinematical theory led somewhat later to the structure factor which shows how the intensity of diffraction is modified by the internal atomic arrangement in the cell and which is, therefore, the clue to crystal structure determination. In the first paper this was not yet recognized; zinc-blende is assumed to have one molecule per unit cube, and therefore the indices assigned to the diagrams and the wavelengths calculated are wrong. Laue is quite aware of the hypothetical nature of the assumption made and stresses the fact that only the ratios of the indices and wavelengths are firmly established.

In retrospect it is astonishing to remember how much thought and energy went into geometrical problems of indexing which now appear quite trivial. The writer, on learning of Laue's experiment, discussed the formulae for a wave-field in a trebly periodic medium which he had developed for light waves but which held also for short waves, and was led to an obvious geometrical construction applicable to X-rays involving a 'reciprocal lattice' and a 'sphere of reflexion'. The formulae were restricted to the case of an orthorhombic cell, and since all crystals used for diffraction at that time were cubic, there was no need of introducing oblique axial systems. The 'reciprocal lattice' soon proved to be a most useful tool in discussing the geometrical properties of diffraction. It was Laue who generalized it for oblique axes, following the definition of reciprocal sets of axial vectors given by Willard Gibbs (but not generally known in Germany at that time). This generalization was made known by Laue at the Second Solvay Conference in Brussels, 1913, the proceedings of which were, however, published only in 1921, after the war. In one of two pre-war papers from Zürich Laue (with F. Tank) took up the discussion of the shape of the spots on Laue diagrams. This shape had, in fact, been the clue by which W. L. Bragg arrived at his concept of the reflexion of the incident ray on the internal densely populated atomic planes. Laue now gave a discussion of this shape in his own terms. The other paper showed for the first time the increase of diffracted intensity obtained by cooling the crystal to liquid-air temperature. This problem went back to the early objections against the experiment which had meanwhile been answered by P. Debye in Utrecht in a series of most remarkable theoretical papers on the effects of heat motion on the diffraction.

Laue's X-ray interest was interrupted by the 1914-18 war and by the subsequent changes of his domicile and position. Two papers on diffraction by mixed crystals (1918 and 1925) deal with the effect of a statistical replacement of one kind of atom in a crystal by a heavier one and the problem of superstructure lines. The quantitative discussion of the intensity of superstructure lines as a function of increasing order in the replacement cleared up misconceptions in this subject so vital for metallurgy. Another often quoted paper of this period (1926) is the one on Lorentz factors and the distribution of intensity in Debye-Scherrer rings, in which the size of the particles of a powder is related to the width of the diffraction rings under slightly different assumptions than those used by Scherrer in an earlier paper.

In 1931 Laue re-discovered for himself the 'dynamical' theory of X-ray interference, the beginnings of which go back to an inspired paper by C. G. Darwin of 1914 and which was fully developed by the writer in 1917. The gist of this theory is that the overwhelming intensity of the undiffracted or primary ray, which was a basic assumption in formulating the kinematical theory, results from the fact that it contains *all* incident wavelengths whereas for incident monochromatic radiation the diffracted rays may have comparable intensities to that of the primary ray. Each atom in the crystal is therefore subjected to an 'optical field' consisting not only of the primary, but also of all secondary or scattered waves in the crystal, and it is this surrounding field which it scatters. The condition of self-consistency between the scattering by the atoms and the field generated by their scattering puts a restriction on the kind of field that can exist in the crystal. This finds its geometrical expression by the requirement that a point, representing the field in reciprocal space, lie on a certain surface, called the 'surface of dispersion'. This surface then determines the modes of all possible X-optical fields in the crystal, just as the dispersion curve for water waves expresses the dynamical condition for waves to travel along the unbounded surface of water. In the case of a crystal bounded by a surface there exists an incident ray, and boundary conditions; this necessitates picking out a suitable set of the dynamically possible optical fields to match the given external field, and eventually the amplitudes of all the waves contained in the optical field are thus expressed in terms of that of the incident wave. The detailed discussion then leads to reflexion curves and expressions for the integrated reflexion which can be compared to experimental results.

In the writer's version, this theory was expressed by considering the atoms as point-scatterers. Laue adapted the theory in a rather formal way to the case of continuous volume scattering and his pupil M. Kohler showed in 1935 how the basic equations could be derived by perturbation theory in terms of the wave-mechanical state of the crystal. Since then Laue and his co-workers used their version of the dynamical theory in discussing the X-optical phenomena. The results of their numerous papers were incorporated in Laue's book *Röntgenstrahlinterferenzen*, the first edition (1941) containing 367 pages, and the third, issued a few days before Laue's death, 476 pages. In this book, and the supporting papers, are to be found Laue's most important original contributions in the field of X-ray optics in crystals. They are often based on elegant mathematics in the style of the classics of mathematical physics like Kirchhoff or Rayleigh. The discussion of the interference effect obtained by Kossel, Loeck and Voges by generating X-rays in the interior of a crystal and observing their exit pattern on a film parallel to the crystal surface is a good example of Laue's craftsmanship. By means of an optical reciprocity theorem, long known but rarely applied, Laue transforms the awkward originally given problem of finding the optical field generated by a point-source at Q in the crystal and following it to the point of observation X far outside the crystal to a discussion of the well-known field of the dynamical theory, which a point source at X produces in the crystal at Q. The reciprocity theorem then states that the amplitude obtained at Q by emission from X also indicates the amplitude obtained at X in the reverse transmission of radiation from the atom at Q.

A most interesting field of application of the dynamical theory was opened up in 1941 by G. Borrmann, one of Laue's co-workers, by discovering the anomalously low absorption of X-rays when diffraction takes place. Laue worked on this problem during his confinement in Farmhall and took the manuscript of his first paper on this subject to the first Assembly of the International Union of Crystallography at Harvard. The formal extension of the dynamical theory to the case of absorbing crystals had been undertaken nearly twenty years earlier when J. A. Prins discussed the reflexion of X-rays by fairly perfect calcite crystals. But neither he, nor Zachariasen, Parratt, Renninger or others using the same theory had given a discussion of the intricate nature of the optical field in the crystal when a primary and a secondary wave coexist. Laue showed that there is a standing wave component with nodal planes parallel to the reflecting atomic net planes and that absorption diminishes as the nodal planes move over the strongly absorbing atoms when the direction of incidence is (very slightly) changed.

In subsequent papers connected with the Borrmann effect Laue approached the theory of diffraction in an entirely novel way by considering the energy flow (Poynting vector) instead of the wave amplitudes. It seems quite likely that this form of discussion will become increasingly useful in the future. It has not often happened in the history of science that the author of an important discovery has added a new slant to its interpretation nearly fifty years later.

The theory of electron diffraction has to follow the lines of the X-ray dynamical theory because of the much stronger interaction of matter with electrons than with X-rays. The theory was developed by H. Bethe in 1928 using many of the concepts and constructions already introduced in the writer's X-ray theory. The main new feature of the electron case lay in the fact that the expression of scattering could be obtained by perturbation methods from the wave-mechanical ground state of the crystal and that it implied scattering on the periodic electric potential distribution, which is continuous, rather than on discrete atomic scattering centres, as was assumed in the X-ray case. This made a Fourier development of the potential necessary, and the basic equations are expressed in terms of the Fourier coefficients. This procedure is so close to Laue's formulation of the X-ray dynamical theory three years later that it is likely to have served him as an example. The similarity of method as for X-rays and electrons also accounts for Laue's intensive work on the theoretical aspects of electron diffraction from about 1936 onward. This work was consolidated in 1944 in book form in *Materiewellen und ihre Interferenzen*, a companion volume to that on X-ray diffraction. A second edition appeared in 1948. The book again impresses the reader by its thoroughness and mathematical detail; though having much in common with the X-ray book, it is complementary to it, rather than repetitive.

OTHER CONTRIBUTIONS TO PHYSICS

Laue's name will always loom large over any chapter dealing with diffraction; but also in other fields of physics his contributions assure him a lasting place in the history of their development. Most of his work lies in the fields of relativity and thermodynamics, but there exist a variety of occasional papers on subjects brought to his notice by his scientific friends, or, like the group of papers on the physics of electrons in amplifying tubes, by his co-operation with W. Wien's group in Würzburg during the first world war.

The characteristic of all these papers, as of his books, is the clear formulation of a well-defined mathematical problem, its solution, and the discussion of the solution in view of experimental results. The reader can rely on the correctness of the consequences drawn from the assumptions put forward and thus check the latter on the experiments. Of each and every paper of Laue's could be said what he claims to be the spirit in which he wrote his book on X-ray diffraction: 'The book . . . does not spare the reader, not even mathematical proofs either tedious or hard. For nothing would be more disastrous to our science than if physicists got used to putting up with *beliefs* when *proofs* are necessary and possible. This book aims at *convincing*—not of its containing nothing but absolute truth, but of the fact that our present physical understanding, if logically applied, leads to the stated results. And some of this, at least, may survive the change of times.' It is by this analyzing and clarifying tendency that Laue exerted his great influence on the developing fields of relativity and, later, of superconductivity. There are, however, quite a number of papers containing an original approach and important, previously unknown results which have to be mentioned separately. Thus the early papers defining the degrees of freedom of radiation and their relation to coherence are quite fundamental. The same is true for those on the thermodynamics of an electron gas and the distribution of potential in amplifying tubes—a problem which Langmuir had treated, unknown to Laue during the war, in a rather incomplete way.

In the field of relativity, the extension of relativistic dynamics to continuous media (1911), and in 1923, the construction of a strict solution of the general field equations given an arbitrary distribution of stars, as well as the discussion of Hubble's red-shift may be considered as outstanding work.

In superconductivity, Laue's discussion of the threshold value of the magnetic field has been mentioned above. When Shoenberg's measurements proved that mercury crystals present different penetration depths of the magnetic field on different faces, Laue showed how to account for this by substituting a tensor λ_{ik} for the scalar parameter λ by which F. London had expressed the relation of momentum to current density for a supercurrent. Later, in 1949, he substituted for this linear relation a quadratic one, following Koppe's (rather different) attempt at introducing a maximum supercurrent density. Laue's studies were incorporated in his book on superconductivity of which editions appeared in 1947 and 1949 and an American translation in 1952.

In an entirely different field Laue was also very successful. A book of some 170 pages on the history of physics took shape in the Hechingen period as a distraction from the ruinous last stages of the war. It is based on his lifelong interest in the development of ideas and concepts in physics, a subject to which much of his reading was devoted. The latest, fourth, edition, appeared in 1959; by then translations into French, Russian, Polish, Japanese, and English had been published.

PERSONALITY

Max von Laue was appreciated by his contemporaries for his character as much as for his brains, and for the combination of both. For many decades those who knew Laue—and this was a large and varied group—might react, on hearing of some new scientific, administrative or political development, with the quick question: what is Laue's opinion on it? Not that Laue always formed an opinion, or made it known; but *when* this was the case, it was well-reasoned, mature, and responsible. He was keenly interested in national and cultural politics and a follower of the Stein-Hardenberg tradition that shaped the spare, hard-working, utterly reliable, protestant Prussian official, to whose devotion Prussia owed much of her greatness. Laue's love and admiration of Prussia was deeply engrained in his personality, and it meant an obligation to him. His sense of justice and fair play was keenly developed and he never failed to stand up for what he thought was right. In the Hitler days it was not without the risk of personal bodily injury and official reprimand to defend even scientific views like the theory of relativity against the science marshals of Hitler, Lenard and Stark. Or to attend the funeral of a Jewish friend, Dr A. Berliner, editor of *Naturwissenschaften*, at the Jewish cemetery in Berlin. Yet Laue took all these functions on himself. When

Einstein resigned his membership of the Berlin Academy the then Vice-President saw fit to issue a statement to the effect that this was no loss to the Academy. Laue, as far as is known, was the only member to send in a letter of protest. At no moment did this political courage degenerate to bravado; it came as the result of a deep feeling of responsibility for national and human welfare and from a pool of despair over Germany's decline.

Laue could enjoy a joke and would quite often produce one himself with an outburst of his own boisterous laugh. Not long ago Gerlach, examining a medical student, asked him whether the Munich tramway ran on d.c. or a.c. The student guessed a.c. Why, asked Gerlach. Because the tram changes direction, was the answer. Laue, on being told this story, burst out: 'He got the frequency wrong!'

Laue was fond of sailing, mountaineering, skiing, and motoring. He wrote the first edition of the book on relativity on the platform of his boat-house on Lake Starnberg, south of Munich, and states that never again did he work under such ideal conditions. His mountaineering took him to many of the high peaks in the Alps, generally in the company of scientific friends, such as the approved alpine guide Hermann Mark. In contrast to Planck who was a well-known rock climber, Laue preferred touring on glaciers. Wearing a rather old-fashioned costume and never without a heavy Zeiss glass dangling from his neck, Laue would toil up, often telling stories or even discussing physics. Once on top, however, his attention belonged undivided to the peaks around, and he would not be satisfied before having identified, with his glass and the map, whatever was to be seen. In contrast to most tourists Laue never took to photography.

Laue's motoring was quite famous in Berlin. He was living at the city's boundary, in Zehlendorf, and for many years he went to the University at the centre of Berlin on his motorbicycle, at high speed, and usually preoccupied with his lecture. He was, at that time, probably the only professor riding a motorbike. Later he owned a convertible Steyr car which became just as famous as its predecessor. In spite of fast driving, Laue drove safely and without accident, except for the fatal one for which the responsible party could not be ascertained.

In spite of his great success as a scientist, and the many honours bestowed on him, Laue was not spared depressive moods which, in his later years, took the form of a feeling of being persecuted, be it by scientists, be it by the military whom he abhorred. In his published autobiographical essay (1952) Laue hints at a great shock he received when a young man, and this may well have been a psychic trauma the effects of which flared up repeatedly during his life-time, assuming various shapes. He fought them valiantly and, having overcome them, regained his full mental vigour, his warmth of feeling and friendship, and pleasure in life.

Though not active himself in any form of art, Laue was appreciative of many of the arts and was especially fond of classical music. His fine sense of his native language and intimate knowledge of the German classics lends distinction to the numerous historical addresses and essays, and human appreciations which came from his pen, all of them based on an intensive study of the history and philosophy of science. Like Kant, Laue looked in awe and humility at the starred skies, the alpine panorama, and at the performance of man's mind. Though he rarely spoke of it, he was deeply religious in a personal way. He asked that his tombstone carry the words that he died firmly trusting in God's mercy.

Στείχομεν οἰκτροὶ καὶ πολύκλαυτοι,
τὰ μέγιστα φίλων ὀλέσαντες.

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K. Linderstrom-Lang

KAJ ULRIK LINDERSTRØM-LANG

1896-1959

KAJ ULRIK LINDERSTRØM-LANG was born on 29 November 1896 in Frederiksberg, Copenhagen. His father, Carl Frederik Linderstrøm-Lang was a teacher of German and Latin at the Frederiksberg Gymnasium. The name of Linderstrøm came into this family from Lang's grandmother, whose maiden name was Johanna Ulrika Linderstrøm. The mother, Ellen Hedvig Bach, came from a family with certain artistic inclinations, while many of Lang's forefathers had devoted themselves to learned professions, chiefly as high-school teachers. A cultural atmosphere prevailed in the parents home and there was much intercourse with academic circles.

Kaj Linderstrøm-Lang was a very talented youngster with a great variety of interests, especially in the artistic fields. He painted, played the violin and wrote poetry, but also devoted himself to practical work. These interests came to play a role during his future life. I remember how he could suddenly interrupt his work in the laboratory, walk across to his home and take to his violin, still in his lab-coat. Among the great variety of interests which manifested themselves in Lang's early youth, science did not seem to stand out particularly, and at school he was evidently not recognized as particularly outstanding.

Nevertheless, at the age of 17 he began studies at the Polytechnicum of Copenhagen (Polyteknisk Laereanstalt), specializing in chemistry. Two years earlier Lang had lost his father, and as a result the financial situation of the family changed considerably. It appears that Lang's interest in chemistry at this time was not so much founded upon a desire to make a scientific career as on the necessity to make a living. Nevertheless, he devoted himself also to the gayer aspects of student life, where his many talents flourished, and he made many friends. The diversity of his interests—at this time he wrote several plays—led to a certain restlessness which culminated in a crisis which forced him to stay away from his studies for half a year. Finally, however, he graduated (1919) and his choice of project as a part of his examination—from the field of ceramics—may perhaps reflect a vague attempt to unify his artistic and technical interests.

To many of us who came to admire Lang's unique personality and great charm, which came to expression also in purely scientific discussions, it is well to remember that at the bottom of all this were bitter experiences and a disharmony which he only succeeded in mastering with great effort.

After a brief employment as a chemist at the Royal Veterinary and

Agricultural College in Copenhagen, Lang was recommended by his chief, Professor A. C. Andersen, to apply for a position as research assistant in the Chemical Department of the Carlsberg Laboratory. He was appointed to this post in August 1919. Evidently, Professor Andersen and also the well-known pharmaceutical chemist, Professor Baggesgaard-Rasmussen, one of Lang's teachers at the Polytechnicum, had realized his exceptional talents, and gave their warm personal recommendations to Professor S. P. L. Sørensen, the head of the Carlsberg Laboratory.

This became a turning point in Lang's career and—still more—in his life, which he now gradually could bring into harmony around a deep devotion to scientific work. It is obvious, also from Lang's own statements, that a decisive role in this development was played by S. P. L. Sorensen personally and the scientific atmosphere which he had created at Carlsberg. For a person who—like the author of these notes—has had the privilege of the friendship of both these great men, this may seem somewhat unexpected. They were very different indeed, both in temperament and the attitude to scientific work, quite apart from the difference in general outlook which was due to the difference in age. Sorensen was, of course, at this time already a world-famous man, known particularly for his work on proteins, enzymes and for his fundamental investigations into the importance of acid-base equilibria (pH scale) in such systems. Lang was a beginner, still feeling his way and very uncertain of himself, but he was rather more inclined to speculation and to theoretical work than one would would have expected to be likely to fit in with Sorensen's painstaking experimentation which often involved extensive routine analytical and preparative work, requiring much patience. Under Sorensen's daily supervision Lang learned this patience, however, and he realized that it did him good. Sorensen, on his side, evidently came to understand that there was much more in the young man than is normally required of an ordinary assistant, and he gradually gave Lang much freedom to follow his own ideas, and to take part of the responsibility for directing the research work at Carlsberg. Lang never forgot his great indebtedness to his master and often expressed it both publicly and privately. Lang's strong inclination to physical chemistry brought new and fruitful aspects of many problems studied at Carlsberg and here he had much benefit from contacts with the very active school of physical chemistry of solutions which flourished in Copenhagen at this time, with men such as Bjerrum, Brönsted and Christiansen.

Lang's first scientific paper (1921) was of physico-chemical character and dealt with the 'salt error' of the quinhydrone electrode. It was published jointly with S. P. L. and Margrethe Sorensen. The problem was of considerable practical importance in the determination of pH but also stimulated Lang to a rather deep-going thermodynamic study of 'salting out' effects in general (1923). He had now found his own way, he saw the great possibilities in the application of physico-chemical laws to biochemical systems such as had been studied at the Carlsberg laboratories from other viewpoints. This strengthened his self-confidence and there was no longer any doubt in his mind that a whole-hearted devotion to science in his own way would give him satisfaction and harmony. Already in 1924—one year after the publication of the Debye-Hückel theory of electrolyte solutions—Lang published a paper on the ionization of proteins, which is rightly considered to be classical. It is fundamental for the interpretation of titration curves of proteins and led among other things also to the important distinction between the 'isoelectric' and 'isoionic' points. Lang returned to these problems also in later years. In parallel with these and other investigations of physico-chemical character Lang also took part in the work on the preparation of pure proteins which at this time particularly attracted Sørensen's interest. Sørensen, struggling with the exceedingly difficult serum-protein systems, had been forced to a hypothesis of proteins as 'reversibly dissociable systems' which would explain some of the preparative difficulties. Lang, on the other hand, chose to study the fractionation of casein and demonstrated conclusively the existence of several distinct individual fractions. There appears to have been a slight controversy over these questions which may have played a role when Lang preferred to drop these complex problems and turn to other fields. Nevertheless his work on casein formed the subject of his doctoral thesis, published in 1925.

Another important result in this work was a new method of determination of free amino groups by titration in acetone. This 'Linderstrøm-Lang titration' made his name widely known and came into extensive use.

It also formed a bridge when he now turned to another field—the proteolytic enzymes, their specificity and their mode of action. A start was made by a brief visit to Willstätter's laboratory in München. After returning to Carlsberg Lang undertook to investigate peptidases and succeded in demonstrating that their specificity is due to the *configuration* of the substrate and not (as earlier assumed) to its chain length. This result, which met some opposition, was later confirmed and amplified by others, especially by Joseph Fruton.

Lang's interest in enzymes took a somewhat unexpected but most interesting course when a young Austrian, Heinz Holter, in 1930 arrived at Carlsberg to work with Lang. Holter had the idea to try to work out a kind of 'enzymatic histochemistry', studying the occurrence and distribution of enzymes in tissues and cells. Lang, realizing the importance of such an approach also from a biochemical point of view, became enthusiastic and in this he strongly emphasized the necessity of a quantitative approach. Thus a very fruitful collaboration was started between these two, where Lang, among other things, contributed by the elaboration of delicate, quantitative ultra-micromethods which in some cases even were capable of application to single cells. Thus micro-titrations were worked out for estimating protein-carbohydrate- and fat-splitting enzymes in quantities about 1000 times smaller than it had earlier been possible to deal with. The extremely elegant micro-gasometric method based upon the 'Cartesian diver' and a highly sensitive method for determining density changes accompanying chemical reactions in small droplets, suspended in a liquid density gradient, were both published in 1939. These methods have come into general use and Lang, Holter and many of their Danish and foreign collaborators used them for a number of important studies. The simplicity and almost artistic beauty of these methods gave Lang much satisfaction and they represent some of the most characteristic pieces of work which we owe to him.

With the aid of these and other methods a number of histochemical problems were studied by Lang, Holter and their many collaborators. Only a few examples will be mentioned here. Thus the distribution of peptidase in germinating barley and the distribution of enzymes in the secretory glands of the mucous membrane of the stomach were studied in great detail. During the period 1931 to 1940 no less than 33 papers appeared as a series entitled 'Studies on enzymatic histochemistry' but certainly many later papers from the Carlsberg laboratory can be counted as a continuation of this work.

When Lang was awarded a Rockefeller Fellowship he chose to go to Thomas Hunt Morgan's laboratory in Pasadena—a further sign of his growing interest in biological problems. During his travel in the U.S.A. he took the opportunity to visit many laboratories, to renew old friendships and to establish new ones. He was welcome everywhere. To Lang personal contacts and discussions meant even more than books and journals in the inspiration and elaboration of new ideas. He was a very good listener and very generous in disclosing his own plans even at a preliminary stage. He managed to combine a speculative mind with a very sound 'matter of fact' critical attitude. Thus he was an ideal participant at conferences and symposia and a delightful companion in informal talk.

In 1938 Lang was appointed successor of S. P. L. Sorensen as Professor and Head of the Chemical Department of the Carlsberg Laboratory. As to his scientific activities and responsibilities this did not involve a radical change since he had already been the actual leader of a large part of the scientific production from the Laboratory for a considerable period. For Lang and his wife the move to the Director's Residence meant that they could now fully develop the hospitality to friends and visitors, which formed such a delightful part of things one remembers as characteristic of the Carlsberg atmosphere.

The war and the German occupation of Denmark meant a curtailing of much of the scientific work at Carlsberg. Foreign guest workers could not come and the situation was naturally very strained. Lang participated in the resistance movement and is said to have been particularly active and successful in rescuing exposed persons to Sweden. He was a brave and outspoken man, and it is not surprising that he was finally arrested by the Gestapo who had to release him, for lack of evidence, after fourteen days. His daughter's fiancé, Peter Fyhn, however, was shot in April 1945 as one of the last victims of the occupation—a great tragedy.

During the war (1942) a special sub-department for cytochemistry with Holter as leader was established at Carlsberg. Lang, who felt that the histochemistry was now in good hands, returned to his protein investigations, especially to a study of the initial state of proteolysis by enzymes. Experimental support was provided for the hypothesis that enzymes preferentially attack a 'denatured' form of the protein, which would exist in equilibrium with the native protein. A particularly important approach to these problems was made in a paper on the contraction accompanying the enzymatic breakdown of proteins (1941). By using a very sensitive dilatometric method it was shown that the rupture of the first peptide bonds causes a much greater volume change than that which accompanies the later stages of hydrolysis. In close connexion with this work came a most interesting discovery (Lang & Ottesen, 1948): 'plak-albumin', a modified ovalbumin gradually formed by the the action of bacterial enzymes on the normal albumin, which had been stored in the ice-box for several years. This discovery and the detailed elucidation of its mechanism was not only important in itself but has a general significance in relation to numerous similar phenomena in protein biochemistry.

The investigations of proteolytic breakdown mechanisms led Lang again into the problems of protein structure in general. The Carlsberg investigations and many others had gradually disclosed that it is not only the 'primary' structure (that is to say the amino acid sequence in the polypeptide chains) that matters. The 'secondary' and tertiary' structures are specially fundamental when it comes to an explanation of the specific biochemical functions of proteins. These structural characteristics are more elusive and difficult to study, but two methods applied at Carlsberg proved extremely useful for this: optical rotation and deuterium exchange. Optical rotation often undergoes a radical change on denaturation, and it seems particularly that rotation dispersion measurements may yield important information about the mechanism of these reactions. The deuterium exchange which occupied Lang a great deal in later years provided an extremely simple and elegant method of distinguishing between exchangeable and firmly bound hydrogen atoms in the protein structure, which evidently reflects the kind of structural changes which are involved in different transformations of the molecule. In these studies the gradient densitometric method already mentioned came to extensive use.

This and related investigations placed Lang and his group in the focus of the interest in the important problems of protein structure, which have attracted so much attention in recent years. Lang was a central figure in most discussions and symposia in this field, and several stimulating reviews of these problems were contributed by Lang, for example 'Proteins and enzymes' (Lane Medical Lectures, 1951) and 'Protein structure and enzyme activity' by Lang and J. A. Schellman (1959).

I have attempted to give a review of some of the most important works of Kaj Linderstrøm-Lang and his many collaborators trying to demonstrate also their background. In Lang's case I believe one can say that much of what he did and certainly the best he did was an expression of a personality of unusual dimensions. The lasting value of his contributions to science cannot be adequately measured only by what one can read in his printed publications, even if these are of considerable weight. His stimulating generosity in the exchange of ideas and his inspiring personality created among his numerous collaborators from many countries and among people who had an opportunity to meet him at discussions a fertile ground which continues to give rich harvests which cannot be measured.

Most leading scientists nowadays have to assume administrative duties, and Lang naturally had his share of these. He was hardly enthusiastic about this sort of work, but he assumed it as a duty and did it well. Thus he was, among other things, President of the Danish Academy of Technical Sciences (1956) and was elected President of the International Union of Biochemistry (1958).

He was elected Foreign Member of the Royal Society in 1956. Among his many other distinctions may be mentioned membership of the Danish, Norwegian, Finnish and Swedish Academies of Sciences, of the National Academy of Sciences in Washington and the Academy of Sciences of the USSR as well as of numerous other scientific societies. He received honorary degrees at the universities of Gent (1948), Stockholm (Caroline Institute) (1948), University of Cambridge (1949), Copenhagen Royal Technical College (1954), New York University (1955), University of Copenhagen (Doctor of Medicine) (1955), University of Oslo (1956).

Lang was married in 1922, a few years after he had joined the Carlsberg Laboratory. His wife, born Gerda Kyndby, was a schoolteacher, and also came from a family with teaching traditions. The generous and unassuming hospitality, an unforgettable element in the 'Carlsberg atmosphere', was natural to her. She never failed to do her utmost to meet all demands on the Lang family in these and other connexions. She often accompanied her husband on his many foreign trips and faithfully shared his happiness and worries in his manifold activities and duties.

At 50 years of age Lang contracted a mild form of diabetes. In March 1958 he was operated on for a relatively benign tumour of the intestine, which could be removed completely. Diabetic complications appeared, however, and he gradually became much weaker. He died on the 25 May 1959.

In human judgement Kaj Linderstrom-Lang left us far too early. By his numerous colleagues and friends all over the world this was felt as a particularly tragic loss—it was almost as if one had lost a member of the family. This came to expression in a most emphatic manner in various manifestations from all parts of the world. We shall always miss him and we shall never forget him.

In preparing this biographical note the author has used some already published material, especially an obituary speech delivered by Heinz Holter before the Royal Danish Society of Sciences on 15 January 1960 (published in: *Oversikt over Det Kongelige Danske Videnskabernes Selskabs Virksomhet 1959-1960*, March 1960).

A. Tiselius

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Guy A. K. Marshall

GUY ANSTRUTHER KNOX MARSHALL

1871-1959

In his great 'History of applied entomology', published in 1930,* Dr L. O. Howard, for many years the most distinguished leader in this field, remarks on the apparent inability of academic men to realize the scientific interest of entomological studies having any practical significance. This attitude, he suggests, is indicated clearly enough by the failure of the great scientific academies in various countries to elect economic entomologists, as such, to their fellowships. He points out that although Antonio Berlese and Filippo Silvestri in Italy were early elected to the Academy of the Lincei and Paul Marchal in France to the Academy of Sciences, this was due to their achievements in pure entomology, that is, as a branch of zoology. Dr Howard himself was not elected to the American National Academy until his 59th year. 'I have a suspicion', he adds, 'that, if it had been thoroughly understood by the members of the academy that he' (the writer) 'was so pronouncedly utilitarian in his work and his views, he might have failed.'

This attitude has not greatly changed. It may even be said, that from the standpoint of the entomologist, it has changed rather for the worse. There was a time when the great Academies looked with interest on the work of the entomological taxonomists on which the whole structure of entomological science is erected. They are still interested in investigators who use insects as material in genetics, physiology or biochemistry; but they are, in general, little interested in the entomologist who devotes himself to insects as insects. Perhaps this attitude has not notably impeded the advance of entomology; but it does tend to separate the Academies in a rather regrettable way from one of the main currents of biological work in which their influence would be beneficial and from which they themselves might draw some useful lessons.

It is therefore a pleasure to record that the Royal Society of London, by its election of Guy Marshall as a Fellow, in 1923, recognized the important part he had already played in the development of applied entomology in the Commonwealth and thus provided him, during the many years of active work that were to follow, with the strong backing the Fellowship of the Society carries. I suspect that the arguments for Marshall's election were drawn from his research in pure entomology and had to do chiefly with his experiments in relation to the theory of mimicry; but I do not wish to stress this point, preferring for once to say, that the end justified the means.

An adequate obituary notice of Guy Marshall is difficult to write. He left no record of his life and works in the archives of the Royal Society. His entry into the entomological profession was most unusual and his whole career presents to the present writer—whose upbringing and antecedents have been altogether outside the English scene—one of those characteristically English puzzles in which the conventional and the unconventional are peculiarly but happily combined. Certainly it would be a caricature to label Marshall with his mental stability and his perennial good humour, as one of the great English eccentrics; but one can at least classify him as one of the last of an almost extinct biological species: the gentleman amateur.

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* *Smithsonian Misc. Collections*, Vol. 84.

Marshall is a common enough name. The remote origin of Guy Marshall's family is for the present rather obscure and this obscurity is not dissipated by a family legend attaching the line to a mythical ancestor in Scotland. However, the genealogy as substantiated by facts, is quite sufficiently distinguished. The grandfather of Guy Marshall was the Reverend Prebendary William Knox Marshall, Honorary Canon of Hereford and Vicar of Wragby, in the County of Lincolnshire. Louisa Maria Chowne, the daughter of the Honorary Canon, married Sir Robert Anstruther of Balcaskie, Fifth Baronet, Lieutenant-Colonel, Grenadier Guards and Lord Lieutenant of Fife. The Honorary Canon also had two sons, Major-General George Frederick Leycester Marshall, C.I.E., R.E., and Colonel Charles Henry Tilson Marshall, Guy Marshall's father. Colonel Charles Marshall married Laura Pollock, daughter of Sir Frederick Pollock, First Baronet and Chief Baron of the Exchequer. The Marshall family possessed a coat of arms which, judging from its appearance, is a modern grant. On a bookplate of the arms, the family is described as 'Marshall of Manor Cunningham'. There is, in fact, a locality in County Donegal called Manor Cunningham. These indications confirm my faint recollection that Marshall mentioned Northern Ireland to me as the original home of the family and though no Marshall of Manor Cunningham appears in the edition of Burke's *Landed Gentry* accessible to me (1892) yet everything suggests a family established for a good many generations at a very respectable level. Guy Marshall's father became a district judge in India and his uncle, the Major-General, rose to be Chief Engineer and Secretary to the Government of the Punjab.

These two distinguished officers were also naturalists. Guy Marshall's father was joint author with Allan Hume of *The game birds of India, Burma and Ceylon* in three volumes (1879-1881); while his uncle wrote on *Bird nesting in India* and on the butterflies of India, Burma and Ceylon.

Guy Marshall was born in Amritsar, Punjab, on 20 December 1871. Like most Anglo-Indian children, he was sent to school in England at an early age, yet the interest of his father and uncle in natural history and more particularly in entomology, must have affected his outlook. It is said that he was encouraged in these activities by the German headmaster of his preparatory school at Margate, where he began to collect butterflies. However in Charterhouse, to which he was duly transferred, butterfly collecting was apparently regarded as an eccentricity so he adopted the less conspicuous hobby of beetle collecting. After completing his public school education, Marshall took the examinations for the Indian Civil Service, apparently under pressure from his father. To his father's disappointment, but, it is understood, not greatly to his own, he failed. I had it on his own authority that he was then packed off by his father, as an unsatisfactory specimen, to a sheep farmer in Natal. On 4 July 1891, he sailed from Southampton on the *Glantully Castle* for Durban, being then only nineteen years of age. For the next fifteen years, the history of Marshall superficially, at least, resembles that of the more energetic type of the exiled 'black sheep'. He went from his sheep farmer to a cattle man; left the cattle man to try mining in Salisbury, Rhodesia, where he worked for a time in a law office. By 1901 he had become the co-manager of the Salisbury District and Estates Company. He and his associate built a store in which C. F. M. Swynnerton, well known for his tse-tse fly work, was employed for a time and about this time also he purchased two farms, one of which Swynnerton managed, before he became a Game Warden in East Africa.

It is difficult to envisage, at this distance of space and time, the conditions of Marshall's life in Africa, but it certainly seems calculated to discourage the most enthusiastic entomologist. However, it did not discourage Marshall. As early as 1896, he is mentioned in the reports of the Hope Department of Entomology and had established relations with Professor Edward Poulton. It was possible to disagree with Professor Poulton, possible not to share his views on the theory of mimicry; but it was impossible not to recognize and applaud his vast enthusiasm for entomology and his willingness to take any amount of trouble to encourage a budding naturalist. Stimulated by Professor Poulton, Guy Marshall carried out a long series of experiments—perhaps the first of their kind—on the significance of insect colours as protections against attack; and the results of these studies eventually appeared in the *Transactions of the Entomological Society of London* in 1902 as a joint paper by Poulton and Marshall. In addition to the record of the experiments on mimicry, this paper contained many field notes as well as a proof of Marshall's hypothesis that supposedly distinct species of the butterfly genus *Precis* are merely seasonal forms.

Four years later Marshall was in London, on his way to Sarawak, where he had obtained, presumably with the help of Professor Poulton, an appointment as Curator of the Museum. However he had developed in Africa a serious malady to which he had apparently paid little attention and this came to a head just when he was due to sail for Sarawak; and required an immediate and severe operation. He was therefore unable to take the Sarawak post and in 1909, after publishing some important systematic papers on weevils he was appointed as Scientific Secretary to the new Entomological Research Committee (Tropical Africa) by the Secretary of State for the Colonies. Dr S. A. Neave was appointed to represent the Committee in East Africa. It was, at first, a very small affair but in the hands of Marshall it developed rapidly into an important organization which served as a model for the agricultural information services organized later by the late Sir David Chadwick and now known as the Commonwealth Agricultural Bureaux. This was a very remarkable achievement for which Marshall, judged by ordinary standards, seemed not at all well fitted. In fact, he had never attended a University and so far as I know, received no formal education in science. He took up entomology as a profession at the age of thirty-eight, having as his only scientific background his part-time work as an amateur—and after spending what we usually consider the formative years in occupations most entomologists would find quite uncongenial.

Nevertheless events showed that Marshall's excellent public-school education, his family background, his equable and friendly temperament and his native intelligence, far more than compensated for the absence of academic training. Coming into the work very much from the outside, never having gone through the Civil Service mill, he brought to it a freshness of viewpoint, an originality and independence which his Colonial Office colleagues may occasionally have found a little startling but which enabled him to see desirable objectives and achieve them with the minimum of negotiation and red tape.

Almost immediately, he perceived the need for an avenue of publication for the Colonial entomologists and founded, to meet it, the *Bulletin of Entomological Research*, now in its 50th year. With the rapid world-wide development of entomological work summaries of current publications were even more necessary and to provide these he created in 1913 the *Review of Applied Entomology*. By that time the Entomological Research Committee had become the Imperial Bureau of Entomology which soon began to receive subsidies from all the countries of the Commonwealth and was thus able to expand its activities. Dr S. A. Neave returned to join Marshall in 1914 and from then on devoted himself mainly to the vital task of organizing and maintaining the staff of the *Review*. The Bureau took over from the Zoological Society of London the increasingly heavy task of preparing the invaluable 'Insecta' section of the *Zoological Record*, which lists and classifies all entomological papers. Very soon, also, the Bureau took up the daring and almost superhuman task of identifying insects sent in by Commonwealth entomologists. Marshall himself was appointed Adviser on Entomology to the Colonial Office and that brought him in touch with the extremely able administrator, Cosmo Parkinson, who later became the Permanent Under-Secretary of State for the Colonies. Marshall then arranged for the appointment of an Honorary Managing Committee, made up of the most distinguished zoologists and entomologists of the United Kingdom. The first Chairman of this Committee was Lord Cromer. The chief entomologists of the self-governing Dominions were made *ex officio* members of the Managing Committee. The organization of the Bureau under these high auspices established its status as a scientific body of major importance in the Commonwealth and this was confirmed by the award of an Honorary Doctorate of Science to Marshall as Director by the University of Oxford, in 1916.

In his administration of the Bureau, Marshall never allowed himself to be hampered by narrow nationalistic considerations. He took men he considered suitable for his work where he could find them. He had at various times a Russian, a Swiss, a Dutchman and Canadians on the staff, and this in spite of some protests from university representatives anxious to find posts for their graduates. I believe I am correct in saying that after the Russian revolution he enlisted the help of the British navy to bring B. P. Uvarov from the Eastern Mediterranean to England and thus gave this brilliant entomologist the opportunity to take up the work which was later centred in the Imperial Locust Bureau. He succeeded in interesting the American entomologist, W. F. Fiske, in the tse-tse problem and this, I believe, was one of the first systematic efforts to deal with this pest which has been such a serious obstacle to the development of Africa. He was also particularly interested in termite control. As the years went on, his numerous contacts as unofficial chief, adviser and friend, with the Colonial entomologists, together with his bibliographical and identification work, allowed him to accumulate an encyclopædic knowledge of world entomology and entomologists. I think his heart was really in Africa, but his interests were universal. Even in Great Britain, his advice was greatly valued. For many years he was the entomological representative on the Research Advisory Committee of the Forestry Commission. Oxford appointed him as one of the Electors to the Hope Professorship of Entomology and he served on the governing body of Charterhouse.

After the first World War the creation of the Empire Marketing Board, provided with ample funds and with broad-minded views about their use, enabled him to realize a long cherished project: the creation of a biological control service for the benefit of Commonwealth entomologists. Thus was founded Farnham House Laboratory, organized and equipped in 1927 by Dr S. A. Neave and assigned to the writer of this article in 1928. From this there eventually developed the Commonwealth Institute of Biological Control which now has laboratories in Canada, the United States, Switzerland, India, Pakistan and Trinidad, W.I., and through this chain of field stations is able to undertake investigations and supply material for practical experiments on a scale never hitherto attained.

Earlier in this notice I mentioned the original type of organization Marshall created for the Bureau of Entomology, with an Honorary Committee of Management composed of distinguished zoologists and entomologists, experienced administrators like Lord Cromer as Chairman and a Secretary attached to the Colonial Office. This arrangement was adopted by the Imperial Bureau of Mycology, modelled on the older organization. It allowed the Director great freedom but ensured a supervision by experienced scientists, who met twice yearly to examine and discuss the affairs of the Bureaux. It kept the Bureaux in contact with the scientific corporation and gave the members of the Committee a personal interest in its work. It provided, Marshall felt, satisfactory guarantees to the countries subsidizing its work. It was therefore a great disappointment to Marshall when, after the creation by Sir David Chadwick of a group of similar organizations, it became apparent that for political reasons, the well-tried arrangements would have to be abandoned and the Honorary Committee of Management replaced by a controlling body of non-technical representatives of the contributing countries. The new Bureaux were specifically forbidden to engage in research; research was a main function of the Bureau of Entomology and Mycology. To stress this difference and in the hope of maintaining their independence, the Directors and Managing Committees of the old Bureaux changed their names to Institutes. This step was, however, without effect and about 1932 the Prime Minister of the United Kingdom dissolved the Honorary Committees of Management and the Institutes were then absorbed by the new organization known first as the Imperial and now as the Commonwealth Agricultural Bureaux, governed by a non-scientific Council representing contributing Governments and having a permanent Secretary as its chief executive officer. I do not think Marshall ever became entirely reconciled to this change but it did in the long run prove beneficial. It committed the self-governing units of the Commonwealth, once they had decided to support the Institutes and Bureaux, to financial support for reasonable periods at the end of which a Commonwealth Conference enabled delegates to examine the work of the organizations, and make any changes they considered desirable. Although the governing body is not, in principle, composed of technical officers, the Conference delegations are. Certainly, the arrangement has some peculiar and awkward features, but it has ensured the survival of the Institute of Entomology through some extremely difficult periods; and specialist conferences, held periodically by the Institute of Entomology, have provided it with the technical assistance formerly given by the Honorary Managing Committee. It seems, therefore, fair to say that from the creation of the Bureau of Entomology due to the initiative and originality of Marshall, there eventually developed a co-operative information and research service which all the self-governing units of the Commonwealth have continued to support because the service provided is obviously in their own interest.

Marshall's personal research, after he left Africa, was, I believe, exclusively taxonomic. He acquired a broad knowledge of all groups of insects in his identification work, but he specialized on the beetles of the family Curculionidae and to the knowledge of this difficult group he made a very solid and extensive contribution, regarded with respect by the other specialists in this field. In common with many of the older entomologists everywhere, he lacked the advanced technical knowledge of specialized fields such as chemistry, genetics, physiology and statistics, but I do not think this was ever felt to be a defect by those who consulted him. He possessed in a high degree a quality of the successful administrator—the art of delegating responsibility—and though he was always willing to help, he seldom interfered.

For various reasons, applied entomology in all parts of the world, has developed as a government service to the public, under the control of Ministries or Departments of Agriculture, Forestry or Public Health. Rather characteristically, Marshall did not feel that this was the only or even the best method to conduct pest control operations. In the first years of the last War, therefore, he set up a private commercial Pest Control Service in co-operation with an Austrian entomologist and carried this on after his retirement as Director of the Institute in 1942.

As Marshall's work on the curculionids was very valuable to the Natural History Museum, he was provided with a room in which to work after he retired and he continued his systematic studies until a few weeks before his death. The severe operation he was obliged to undergo just before he began work in England seemed to have no after-effects and his continued activity almost until his death on 8 April 1959, at the advanced age of eighty-seven, was evidence of his exceptionally good constitution. In 1933 he married Hilda, daughter of the late David Alexander Maxwell and widow of James Ffolliott Darling. With two step-sons, she survives him.

In spite of his many official duties and his friendly disposition, Marshall was a rather retiring man and never sought publicity. He was pressed on many occasions to accept the Presidency of the Royal Entomological Society of London but consistently refused. Nevertheless, the importance of his work was recognized by many honours. He received the C.M.G. in 1920, was knighted in 1930 and advanced to K.C.M.G. on his retirement in 1942. He was elected to the Royal Society in 1923, became a Corresponding Member of the American Academy of Science, Philadelphia, an Honorary Member of the Royal Society of New Zealand, of the National Institute of Science of India, of the Entomological Society of Belgium, of the Entomological Society of Russia and of the Royal Entomological Society of London. He also received from the Belgian Government l'Ordre de la Couronne. These honours were well deserved for the services rendered by Marshall to Commonwealth and World entomology were outstanding.

W. R. Thompson

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Eremninae (Col., Curculionidae) of the Indo-Malayan Region.

This was originally intended as a second volume on Curculionidae in the 'Fauna of British India' series. It is hoped that it will be published by the British Museum (Natural History).

T. G. Mason.

THOMAS GODFREY MASON

1890-1959

Thomas Godfrey Mason was something of a recluse who talked little of his private affairs and of his childhood and education. These circumstances make it difficult to present as full an account of his life as one would wish.

Thomas Godfrey Mason was born at Greystones, Co. Dublin on 24 July 1890; his father, C. W. Townshend, married Miss A. R. Mason a member of a Dublin medical family. Three of his wife's brothers were medical men in Dublin. Thomas Godfrey and his brother and two sisters were largely brought up in Dublin by their mother and by her father. Ultimately they took the latter's name, Mason. Thomas attended a preparatory school, Temple Grove at Richmond, Surrey, and later went to Cheltenham College. Few records can be obtained of his early interests and activities, apart from a known prowess as a long distance runner and an enthusiasm for motor bicycling during the infancy of that form of transport.

That indeed, *inter alia*, caused him to enlist as a dispatch rider in the 1914-18 war. After a year of such service he returned from France to take a commission in the Royal Irish Rifles.

He had entered Trinity College, Dublin, in 1909 and graduated with a First Class Moderatorship and Large Gold Medal in Natural Sciences in 1913. The war years intervened and, after his return, the M.A. degree of Trinity College was conferred in 1917 and B.Agr. in 1918 followed by the award of Sc.D. in 1923.

Personal accounts of Mason's work and activities in Trinity College, Dublin are difficult to get as few of his contemporaries are now living. It is clear that much was owed by him to Professor H. H. Dixon. The prevailing interests of the School of Botany in problems of translocation in plants are clearly the source of Mason's own interest in this subject which was so much advanced by his work.

Mason joined the staff of the Imperial Department of Agriculture in Bridgetown, Barbados, in 1920 and worked on cotton growing in St Vincent. The Imperial Department of Agriculture moved to Trinidad two years later and became ultimately the Imperial College of Tropical Agriculture with T. G. Mason as its first Professor of Botany.

T. G. Mason, however, left in 1923 to work in the Agricultural Department at Moor Plantation in Nigeria. He returned soon afterwards to Trinidad as Plant Physiologist at the Cotton Research Station set up by the Empire Cotton Growing Corporation at St Augustine, Trinidad, close to the Imperial College of Tropical Agriculture. He held this position until the Cotton Research Station closed in 1946.

The succession of moves and changes of employment may have been broadening to the mind and a source of wider experience rather than conducive to development of a research career, but Mason did take the opportunity to return on several occasions to the School of Botany at Trinity College, Dublin. This inevitably meant active discussion with the Professor who had been obtaining estimates of rates of transport of sugars in plant stems (in collaboration with N. G. Ball, now Sir Nigel Ball, Bart). These estimates led Professor Dixon to a view, regarded at the time as revolutionary, that transport at the very rapid rates observed made it highly improbable that phloem could possibly be the channel of transport. Dixon considered that inevitably one was forced to the view that downward transport must be effected in xylem and this was the theme of his Presidential Address to Section K of the British Association in 1922.

This undoubtedly spurred Mason to the study of transport of sugars in the greater yam which was carried out at Moor Plantation in Nigeria using essentially the same type of argument and technique as Dixon had used in his study of transport in potato. Dixon's data showed that sugar solution would have to travel at a rate of 50 cm/h to supply a growing potato. Mason's data for a growing yam indicated a rate considerably faster than this. This study by Mason also brought to light a 'new' anatomical structure which it seems is confined to the Dioscoreaceae—namely, the compact gland-like groups of parenchyma cells called by Mason glomeruli from a superficial resemblance to kidney glomeruli. These structures which are at present in the nodes appeared to intervene between successive internodal runs of sieve tubes and were regarded by Mason as effective blocks of mass movement of liquid in the phloem.

Mason's work on the anatomy of the nodes of the Dioscoreaceae was carried out in Professor Dixon's laboratory in Dublin whilst on leave from his post in Nigeria. The survey was extensive as the gardens of Trinity College had a very considerable collection of species. Mason's collection of slides remained (and probably still does remain) in the School of Botany.

Mason thus began his researches on transport fully supporting the views then developing in the School of Botany in Dublin, that transport in phloem was impossible. Mass movement at adequate rates through glomeruli was deemed to be impossible and transport by diffusion was also apparently excluded.

When Mason took the post of physiologist at the Cotton Research Station, he began the series of researches on translocation in cotton which are a classic of plant physiological study. In this work he had the collaboration of E. J. Maskell and later E. Phillis. The importance of this series of studies can hardly be overestimated. Before they had been begun, there was no clearly convincing evidence regarding the channel or rate of transport of organic materials in plants and the earlier view that phloem was the tissue involved was breaking down in the light of current researches on analyses of quantities of material known to have arrived in storage organs.

Mason and Maskell's great contribution was that they made comprehensive analyses of concentrations and quantities of carbohydrates (and other constituents) in leaves, stems and also in the possible transporting tissues. The work was conducted on Sea Island cotton; it represented in one sense team work in that Mason trained most efficiently a group of technicians to assist in the very extensive series of samplings and analyses which were involved. The first of Mason and Maskell's joint papers dealt with diurnal variations in quantities (and thus concentrations) of water, sugars and polysaccharides in leaves and bolls of cotton and also in samples of standard length of the stem through which transport occurred. It was primarily concerned with the channel of transport. The close correlation of the daily rhythm in sugar concentration of leaves with that in bark and the relative constancy of concentration in wood suggested the bark as channel of transport as the maxima of concentration in bark followed a few hours after the leaf maxima.

The probability that transport of leaf assimilates through the phloem or bark was the cause of the diurnal rhythm in bark concentration of sugars was supported by a series of ringing experiments in which rings of bark had been cut. This brought about concentration increases above and decreases below the ring which interrupted flow. This phenomenon was known or, more strictly, it had been surmised, on a basis of associated growth effects. Surprisingly it had not been clearly substantiated by accurate analyses before the work of Mason and Maskell.

Flaps and strips of bark were separated from the wood after the manner of a study by Münch (*Ber. dtsch. bot. Ges.* **45,** 340, 1927), but whereas Münch merely noted continued growth of tissue so separated, Mason and Maskell carried out analyses which clearly showed the quantity and thus the rate of transport into the flaps of tissue. They established a pattern of movement of sugars from regions of high to regions of lower concentration but more importantly also showed that the rate was some 40 000 times as great as that of diffusion in water.

Mason paid one of his visits to the School of Botany in Trinity College, Dublin, during the course of this work to have friendly and heated arguments with the Professor who may have been shaken in his view that phloem transport was impossible. He certainly was not convinced of the validity of Mason's claim that phloem transport occurred at these high rates as ringing the bark was rightly held by Professor Dixon to damage the external younger layers of the xylem. These arguments probably inspired Mason's flap experiments on the lines of Münch's contemporary work. These experiments did show that the sugar content of normally attached bark and flaps of bark attached at their upper ends maintained a high sugar and carbohydrate content during a 24-hour period whereas flaps cut off at the upper end lost some 25% of their initial sugar content. It is now accepted that transport into the bark flap as postulated by Mason is the explanation, but argument and a variety of other explanations were provided in the School of Botany in Trinity College, Dublin!

Mason and Maskell extended their work using essentially the same experimental design but extending the treatments by a range of surgical operations designed to 'encourage' sugar movement in comparable plants either upwards or downwards. This was achieved by defoliation of either the lower or the upper section of a region in between two rings. The results indicated that movement from the source of supply, the leaves, to the sink or defoliated region did take place in either direction depending on positions of source and sink rather than on anatomical organization.

Other treatments included constriction of the cross-section of phloem by partial ringing or cutting of windows in the bark so that the concentration gradient in the bark of phloem was artificially steepened.

Equally important were studies of concentration of sucrose and other hexoses in different regions of bark in which the relative cross-sectional areas of sieve tubes and parenchyma cells differed. It was deduced from these data that in leafy normal plants the sieve tubes must be the transport region and this region showed sucrose concentrations three times as great as the parenchyma, whereas the parenchyma and sieve tubes had approximately equal reducing sugar contents. In ringed plants the sucrose content of sieve tubes above the ring rose to some five times that of parenchyma.

The upshot of these studies was the recognition that sucrose appeared to be travelling in sieve tubes from regions of higher to those of lower concentration, and when the cross-sectional area of the transporting region is decreased the total amount which passes is decreased but the rate of movement across unit area is increased in association with the increase in concentration gradient across the constriction. The analogy with diffusion was close.

Mason himself did not consider that he had obtained unobjectionable evidence of the role of sieve tubes in transport. He did consider that this had been achieved by Schumacher's fluorescein experiments in Bonn, which he witnessed when he visited Schumacher's laboratory. Mason, however, failed to achieve similar results in Trinidad.

Mason and Maskell continued this work with a corresponding study of movement of nitrogenous substances through the stem. In this case they found that, although downward transport seemed to take place, the concentration gradient of crystalloid nitrogen was negative, a result which conflicted with their view that transport followed a pattern comparable to diffusion. They considered that there was a possibility, even a probability, that a positive gradient of the molecular species actually transported might be masked by large negative concentration differences of other substances or that positive concentration differences in sieve tubes might be masked by negative values in adjacent or neighbouring tissues.

Many studies, however, in which different regions of the bark were analyzed separately and a range of nitrogenous substances estimated failed to give any experimental support for these views that nitrogen transport followed the 'diffusion pattern'.

Mason continued active research on other transport phenomena with his colleague Dr E. Phillis. Transport of nitrogen and of inorganic ions into the corolla before anthesis and export on the following night were studied and the data suggested that the channel of transport was again phloem.

Mason and his colleagues were naturally preoccupied with the problem of mechanism of transport in phloem. They had provided the first satisfactory evidence of the role of phloem and the first estimates of the gross rates at which substances travel. The magnitude of these rates effectively eliminated normal diffusion as a mechanism. Mason, Maskell and Phillis discussed the impact of their data on such hypotheses as the mass flow under a turgor gradient proposed by Münch and that type of mass flow within individual sieve elements which would be provided by protoplasmic streaming. This they also rejected on the grounds that it implies mixing by streaming within each cell coupled with diffusion through the sieve plates and they calculated the rates of such diffusion and the necessary rates of streaming that would be required to maintain observed rates of transport. On their reckoning protoplasmic streaming at the impossible rate of 5·6 cm/sec would have to be provided.

The notion of an 'activated diffusion' which they advanced brought the problem of mechanisms of transport into realms dangerously close to the supernatural. Mason and Phillis showed the dependence of transport on active respiration; they studied the possibilities of specialized water channels in their work on bound water and on the mechanical expression of water from tissues.

To this day no satisfactorily based hypothesis has been advanced, though special techniques such as the utilization of the stylets of aphids which appear to penetrate sieve tubes and use of isotopes are extending and confirming the Trinidad work on the function of sieve tubes and on the rates and direction of transport.

Mason was elected to the Fellowship of the Royal Society on 6 May 1937, but he was never formally admitted nor did he sign the Charter Book. The list of his publications shows that he continued to carry out active research in Trinidad largely in collaboration with his colleague, Dr Phillis. This continued until the closure of the Research Station in 1946 brought these studies to an end. T. G. Mason retired from active scientific research to private life as a cacao and citrus planter living on his estate of Torrecilla about 10 miles from the Research Station. A colleague from the Imperial College of Tropical Agriculture wrote of his retirement from active scientific life and research that 'after 1946 T. G. Mason went out of circulation' very much to the regret of his colleagues and friends.

A friend writes that he was regarded as a competent and skilful agriculturist by his fellow planters. Although somewhat of a recluse he was sociable, generous and always ready to help his colleagues. He was a regular attender at the meetings of the Agricultural Society of Trinidad and Tobago and members greatly valued his help and advice.

So far as I know he had not revisited Europe during the last twenty-five years. He never married, and he is credited with having stated himself that it would be 'unfair to expect anyone to become his wife', a typical example of his rather dry humour.

Thomas Mason died on 22 October 1959 and was buried in the College Cemetery at St Augustine, Trinidad, on 23 October 1959.

My thanks are due to Dr Mason's sister, Mrs G. Blood, of Enniskerry, and to his sister-in-law Mrs S. A. Mason of Wednesfield; also to Dr P. Richards of Imperial College of Tropical Agriculture and Dr F. Hardy of Turrialba, Costa Rica for information and personal details.

T. A. BENNET-CLARK

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B. Mendel

BRUNO MENDEL

1897-1959

BRUNO MENDEL, who died on 23 August 1959 in his home in Bussum, Holland, was elected to the Royal Society in 1957. He was born in Essen, Germany, on 3 November 1897. He came from a Jewish family. His father, Felix Mendel, was a physician who, in spite of his large practice, devoted much of his time to scientific problems and in the years between 1887 and 1922 published over fifty papers, mainly on therapeutic subjects. It is interesting to recall that Bruno Mendel's father introduced the perlingual application of drugs and was one of the first doctors in Germany to advocate intravenous injections as a simple, effective and safe procedure which ought to be widely used by the general practitioner. 'I made over 2000 intravenous injections without a single adverse effect' he could write in 1903 in one of the fifteen papers he published on the advantage of injecting intravenously various drugs in different diseases. He was one of the first to recognize the therapeutic value of a diet low in salt and he re-introduced the use of squill for certain cases of cardiac failure. This remedy had been known for centuries but, due to the introduction of *Folia Digitalis* by Whitteridge, had been neglected and was in danger of being forgotten.

Bruno Mendel inherited his keen scientific interest from his father: they had much in common. His father was a practising physician but with a strong scientific bias. Bruno Mendel was a scientist who lived with and for his experiments, yet always felt himself to be a physician. Although he worked on purely biochemical or physiological problems, he saw them in association with the general clinical aspect. Whenever he discussed the results of his latest experiments—and this he loved to do—he brought up the question of their ultimate clinical and therapeutic value.

There is no doubt that he was influenced in the choice of his profession by his father. As a boy of twelve he wrote in an essay at school that he wanted to become a doctor and to cure cancer. He retained this urge to work on the problem of malignant growth throughout his life. The problem prompted many of his investigations although often no mention was made of it when he finally published the results. At the time of his death he was working on substances which selectively inhibit ali-esterases. By finding a highly selective non-toxic substance of this kind he hoped to be able to prevent malignant growth.

At sixteen during the first few months of the 1914-18 war, after having passed his University entrance examination in Essen, he joined the Red Cross as a stretcher bearer. He had no illusions about the war but, when he was seventeen, he enlisted in the Signal Corps from a sense of comradeship with the boys of his generation. He was on active service until he was wounded in December 1917. When he recovered, he studied medicine in Bonn, Frankfurt, Halle and Berlin, where he qualified and obtained his M.D. He then worked in one of the great teaching hospitals of Berlin University under Professor Goldscheider, and later he had in addition a successful private practice. During this time he introduced the use of leukotropin in the treatment of various inflammatory conditions, and devised a colorimetric micro-method for lactic acid estimation in blood. This is one of the three methods given by Peters and Van Slyke in their book on *Quantitative clinical chemistry* for lactic acid determinations in blood.

During this time he also made the observation that a dilute solution of glucose treated with concentrated sulphuric acid develops a bluish-pink colour, the intensity of which is proportional to the concentration of glucose. This reaction became the basis of his colorimetric micro-method for the rapid determination of glucose in blood.

One day Mendel realized that he was more interested in the lactic acid metabolism of one of his patients than in his general clinical condition. As a consequence he set up a small laboratory of his own in which he worked until he left Germany in 1933. During these years in Berlin he had close, one may say nearly daily contact, with O. Warburg with whom he discussed his experiments. They were mainly on the metabolism of the cancer cell. He discovered that glyceraldehyde completely inhibited the anærobic glycolysis in tissue slices and extracts of malignant tumours without affecting their respiration or the respiration of normal tissue, and further that this inhibition was reversed by pyruvate.

In 1933, the day Hitler declared the Jewish boycott, Mendel left Germany with his wife and three children, and he never returned. He went to Holland and set up a small laboratory in Bussum where he continued his work on the metabolism of the cancer cell. But he felt that Holland, too, was no longer safe. He interpreted the political situation in Europe correctly and in 1936 accepted an Assistant Professorship under Sir Frederick Banting at the Banting Institute in Toronto, where he later became Associate Professor and in due course Professor and Head of the Sub-Department for Cellular Physiology. He remained there until 1950. Few know of the help he gave after 1933 to many young Jewish couples in Germany to enable them to emigrate and start a new life. Or, again, of what he did after 1940 for young German-Jewish refugees who had been sent to England by their parents and, when war came, were interned and sent to Canada.

He and his family were naturalized in Canada in 1939. His son joined the Royal Canadian Artillery and, on D-Day, took part in the invasion of France as an officer of the Intelligence Corps.

Bruno Mendel loved Canada. Yet he was often homesick for Europe. In addition, his position in Toronto, in charge of a department and yet not fully independent, did not really satisfy him. Shortly after the war he returned to Europe for six months on an exchange Professorship in Amsterdam, and he was glad when, a few years later, he was offered the Chair in Pharmacology at Amsterdam University, which had become vacant through the death of Professor Laqueur. He was appointed in 1950 and was looking forward to many visits to London to attend the meetings of the Physiological Society. However, at the end of 1950, it became apparent that he was suffering from a serious illness. He underwent several major operations for regional ileitis; they had to be performed in spite of the fact that a few years before he had had a coronary attack. By carefully arranging his life and gradually giving up all other activities, he was able to continue his experiments and he did so, full of enthusiasm, until the last day when a final coronary attack ended his life.

Mendel's best known work is probably that on cholinesterases, carried out during his stay in Toronto. This work is of particular importance for the theory of chemical transmission of nerve impulses by acetylcholine. It is not sufficient to distinguish between lipases, ali-esterases and cholinesterase. Cholinesterase itself consists of two different enzymes or two groups or families of enzymes, the 'true' and the 'pseudo' cholinesterase, and as Mendel showed, we can only assign to the 'true', the physiological role of the rapid destruction of acetylcholine when released in the body during nervous activity.

Our present view of the two distinct types of cholinesterase is based on work from various laboratories, but the main work was carried out by Mendel and his associates. In 1943, they published, in quick succession, five papers under the common title 'Studies on cholinesterase'. The fundamental advances made in these studies can be summarized as follows:

(1) Methods of purification of cholinesterase preparations were devised so that their activities could be studied without interference by the common aliphatic ali-esterase.

(2) Specific substrates were introduced.

(3) The cholinesterases were classified in two distinct categories, the pseudo-cholinesterase (later also sometimes termed butyrylcholinesterase) and the true cholinesterase, to which later the name acetylcholinesterase was given. The classification was based not on the source of the enzyme, but on differences in their properties.

(4) The use of specific substrates allowed for the first time the quantitative determinations of each enzyme in the presence of the other.

(5) It was shown that inhibitors could be selective for either cholinesterase, and that complete inhibition of the pseudocholinesterase in the body did not result in reactions indicative of accumulation of acetylcholine.

(6) From the properties and distribution of the two enzymes and from the results obtained *in vivo* with selective inhibition of the pseudocholinesterase, the conclusion was drawn that this enzyme played either

no role, or only an insignificant one, in the physiological destruction of acetylcholine.

Mendel's concept of two main types of cholinesterases met with general acceptance. We may say further that without the introduction of specific substrates and without the correct way of classifying the cholinesterases and their inhibitors, the theory of chemical transmission of nerve impulses by acetylcholine might today have been in a state of confusion, or at least in great difficulties.

The two most important advances resulting from these studies, which were later continued in Holland, were the introduction of specific substrates and the recognition of the fact that inhibitors of cholinesterase could be selective for either the true or the pseudocholinesterase.

By not using acetylcholine as substrate, but different substrates for each cholinesterase—acetyl-β-methylcholine for the true, and benzoylcholine for the pseudocholinesterase—he was able to estimate quantitatively the activity of each enzyme in the presence of the other, because the acetyl-β-methylcholine was practically not attacked by the pseudocholinesterase and the substrate for this enzyme was resistant to the true cholinesterase, neither substrate being hydrolyzed by the common ali-esterase. This principle of using specific substrates has become the guiding one for all further modifications and advances in cholinesterase estimations.

The introduction of specific inhibitors made it possible to obtain accurate information about the distribution of the two cholinesterases in tissue extracts and body fluids. The first quantitative survey of this kind which Mendel, Mundell and Rudney carried out made it abundantly clear how misleading had been the previous approach of trying to classify cholinesterases according to the source of the enzyme. Their results with blood are illuminating in this respect. They found that the blood of all species examined contained true cholinesterase, and that in most species it was mainly located in the red cells, but that the plasma usually contained small amounts as well. And finally, in those species in which the red cells contained no true cholinesterase (birds) or only small amounts (cats), relatively large amounts of this enzyme were present in the plasma. Thus the statement often made previously that the red cell enzyme was the one and the plasma enzyme the other cholinesterase, was incorrect.

It is interesting to recall the heated controversy not so much about the results and classification as about the terminology proposed by Mendel for the two cholinesterases. Instead of the term 'true cholinesterase' the term 'acetylcholinesterase' is nowadays more commonly used to indicate that acetylcholine is the physiological substrate for this enzyme. Since the physiological substrate for the pseudocholinesterase as well as its physiological function is still unknown, the various alternative terminologies proposed for this enzyme have not met with general acceptance. When Mendel coined the terms true and pseudocholinesterase, he emphasized a fundamental physiological issue. Originally the enzyme now called the pseudocholinesterase was thought to be responsible for the destruction of acetylcholine in the body. This was the accepted view until 1943. The definition of the term 'pseudo' is false in the sense of 'wrongly held to be' and everyone who has been actively engaged in work on the physiological function of acetylcholine will agree that for a long time the pseudocholinesterase was 'wrongly held to be' the enzyme responsible for the rapid destruction of acetylcholine in the body. It was this physiological issue which was at the back of his mind when Mendel introduced his terminology.

It is no exaggeration to say that practically all histochemical methods used today for the localization of the two cholinesterases in the tissues make use of the two facts firmly established by Mendel and his co-workers, that there exist specific substrates for each of the two enzymes and that each of them can be selectively inhibited.

The study of selective inhibitors of cholinesterase led to the study of inhibitors of ali-esterases. These enzymes are found in animal tissue, micro-organisms, plants and in neoplastic cells. There is probably no cell which does not contain this type of enzyme. Yet its physiological function or its natural substrate is unknown. Mendel has found that a number of organic phosphate esters which are inhibitors of cholinesterases are even more potent inhibitors of the ali-esterase and when he found that a rat is not adversely affected by prolonged and almost complete inhibition of its ali-esterase activity, he asked himself whether in the course of evolution the ali-esterases might not have lost much of their significance for the animal kingdom while retaining their essential role in the metabolism of lower forms of life. This concept led to the discovery in 1953 that selective inhibitors of ali-esterase inhibit the germination and growth of seeds, the growth of human tubercle bacilli and of malignant cells of a mouse lymphosarcoma in tissue cultures, but not of the non-malignant fibroblasts of the same lymphosarcoma or of other normal cells. During the last years of his life he was endeavouring to find non-toxic, more and more potent and more and more specific inhibitors of ali-esterase in order to study their effects on tuberculosis and on malignant growth.

'How does the Ehrlich ascites tumour obtain its energy for growth?' This is the title of Mendel's last paper published in 1957 together with Kemp and again dealing with the problem of the metabolism of neoplastic cells. This paper illustrates well the way Mendel's mind worked and how he approached a scientific problem.

Warburg had shown in experiments with surviving tumour slices that a glucose concentration of about 200 mg/100 ml. was required for maximal glycolysis. The glucose concentration in the ascites fluid, however, was found to be much lower, only 5 to 7 mg/100 ml. So first Mendel and Kemp ascertained that a concentration of 4 mg/100 ml. is sufficient for maintaining maximum glycolysis of the ascites tumour cells. The next problem concerned the source of glucose or the mechanism by which an adequate influx of glucose into the ascites fluid was ensured. Again a simple experiment gave the answer. Ascites fluid was made cell-free by centrifugation, and 5 ml. were injected intraperitoneally into a normal mouse. Three minutes later the fluid was withdrawn; its concentration of glucose was found to have risen from 3 to 17 mg/100 ml. Although this increase would not fully meet the requirements of the cancer cells for maximal glycolysis under aerobic conditions, the tumour cells normally present in the ascites fluid would keep its glucose level low and thus further enhance the flow of glucose through the capillary wall into the ascites fluid. Yet there was still this problem: where does all the glucose come from? Blood and liver of a mouse contain less than 50 mg glucose, but more than 500 mg are consumed in 24 hours by the cancer cells present in 5 ml. of ascites fluid. Again, Mendel and Kemp obtained the answer by a simple experiment. In the ascites fluid of a tumour-bearing mouse the glycolyzing cells maintain a lactic acid concentration of 100 to 150 mg/100 ml. Mendel and Kemp injected 5 ml. of cell-free ascites fluid intraperitoneally into a normal mouse and showed that at least 13 mg of lactic acid were absorbed from these 5 ml. in 24 hours. Most of this lactic acid must be absorbed into the bloodstream and transformed into glycogen by the liver which will then replenish the blood with glucose. Thus the greater part of the glucose glycolyzed by the cancer cells will re-enter the ascites fluid and once more be available for glycolysis. In this way, the ascites tumour cells, like other cancer cells, are able in spite of their very great glucose consumption and in spite of the low concentration of glucose in the ascites fluid to derive the major part of their energy for growth from glycolysis.

His work on cholinesterases and ali-esterases became of great importance to the research on war gases since many of them are potent anticholinesterases but, when approached after the war to carry out secret work on this subject he refused. He held strong views on the question of secret research in peace time.

His mind was always occupied with scientific problems and whenever one visited him he had to tell of his most recent problem, how he intended to investigate it and what experiments he had performed. He could make his ideas come alive even to a layman. I wish I could convey the charm and enthusiasm with which he explained them. But what endeared him most to his friends and colleagues were the warmth of his personality, his capacity for inspiring affection, and a child-like quality which it was impossible not to love. And this intense enthusiasm he also brought into his teaching. Lectures were not a burden to him. He enjoyed conveying knowledge and sharing with the students his own personal approach to a problem. And when he felt he had been successful and the students had responded he was really happy. During his last years when it became more and more difficult for him to lecture regularly, it gave him a deep satisfaction that whenever it became known that he was going to lecture, the lecture theatre would be overcrowded; the students loved to hear him.

How his co-workers felt about him is perhaps best conveyed by quoting some passages written by Dr J. van Noordwijk, the senior member of his staff, after his death:

> 'His contribution to science was not confined to his own work: his keen criticism of the experimental and written work of younger scientists who worked under him contributed significantly to their scientific training. His thorough knowledge of the relevant literature enabled him to base their training on a broad foundation. His criticism was painstaking but never harsh; he was too kindly a man for that.
>
> 'Owing to his failing health, contact with his staff was of necessity more and more reduced to talks about the work. But his interest in their personal and human problems became immediately apparent whenever an important event occurred in their lives. A general conversation at his home—which was, alas, not often possible—was long remembered afterwards by everyone, even when the substance of the talks had been forgotten, because his wide knowledge and interests and the cultured atmosphere of his home made us feel that here was a bit of European civilization at its best.
>
> 'The enthusiasm and energy with which he continued his scientific work in spite of great bodily handicaps has left an indelible impression on all those who were in daily contact with him in the Pharmaco-Therapeutical Laboratory.'

The keen interest his wife shared with him in all his scientific activities and the unity of their marriage were an inviolable source of strength to him. It was this strength that enabled him to carry on as he did to the very end.

I should like to express my sincere thanks to Mrs Mendel for all the help she has given me. Without it I could not have written this memoir.

W. S. Feldberg

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W. H. Mills

WILLIAM HOBSON MILLS

1873-1959

Although William Hobson Mills was born in London, on 6 July 1873, he was fundamentally a Lincolnshire man. His father, William Henry Mills, was an architect, whose biography is given in *Lincolnshire Leaders* (p. 109), by C. A. Manning Press (Jarrolds 1894), and his mother, of maiden name Emily Wiles Quincey Hobson, was the daughter of William Hobson, the owner of one of the chief business houses in Spalding in Lincolnshire. Moreover, Mills's parents moved to Spalding in the autumn of 1873, so that he became a Lincolnshire man in every respect other than that of his birthplace.

Mills was educated first at Spalding Grammar School and then at Uppingham School. It was at Uppingham in the winter of 1890 that he had an accident whilst tobogganing in the snow, severing an Achilles tendon. Mills remarks in some brief biographical notes that this injury 'limited his outdoor activities': this was certainly true during his youth, but those who first met him in his middle age and found how vigorously he cycled and walked in his visits to the fens in search of plants and birds, would scarcely have realized that he suffered any handicap.

He entered Jesus College, Cambridge, in October 1892 and read natural sciences. The injury to his foot, however, caused him to remain at home for the academic year 1893-1894. He returned to Cambridge in October 1894, and obtained a First Class in the Natural Sciences Tripos, Part I, in 1896, and in Part II (Chemistry) in 1897.

Mills now began research in the Cambridge University Chemical Laboratory under T. H. Easterfield (later Sir Thomas Easterfield), who had recently returned from Wurzburg where he had been working under Professor Emil Fischer. Easterfield suggested that they should prepare 2,4-dibenzoylmesitylene, oxidize the methyl groups to carboxyl groups, and finally condense two of the latter with adjacent benzoyl groups (a process comparable to the cyclization of *o*-benzoylbenzoic acid to anthraquinone), and thus synthesize a pentacyclic system containing two anthraquinone systems having one benzene ring in common.

Easterfield, however, soon left Cambridge to take up the Professorship of Chemistry at Wellington, New Zealand, and Mills continued the work alone: the results form the basis of his first scientific publication.

In 1899 Jesus College elected him to a Fellowship which was tenable for six years.

A brief chronological summary of his subsequent career may conveniently be given here, and the various stages then discussed in greater detail.

In October 1899 Mills went to Tübingen to work for two years under Professor Hans von Pechmann, and in 1902 he was appointed Head of the Chemical Department of Northern Polytechnic Institute in North London. In 1912 the tragic death of Humphrey Owen Jones and his wife, whilst mountaineering in Switzerland on their honeymoon, left a serious gap in the organic chemical staff at Cambridge. Mills was then appointed to the Demonstratorship to the Jacksonian Professor of Natural Philosophy (at that time, Sir James Dewar) which Jones had held, and was shortly afterwards elected to a fellowship and a lectureship in natural science at Jesus College. In 1919 he was appointed a University Lecturer, and in 1931 the University recognized the outstanding quality of his work by the creation of a personal Readership in Stereochemistry, which he held until his retirement in 1938.

The Tübingen period, 1899-1901

Although the actual investigation which von Pechmann gave to Mills—on the action of ammonia and hydrazine on chloro- and bromo-coumalinic acid and their esters—was possibly not of very great interest, the two years at Tübingen made a great impression on him, for both personal and chemical reasons. This was clearly shown in a typewritten article, entitled 'Tübingen 1899-1901', which Mills left among his papers and which has not been published. This article, which was written in 1952, contains much interesting information which should certainly not be left unrecorded, and I am quoting it below, deleting only a few passages that have little bearing on chemistry or chemists.

'I went to Tübingen towards the end of October 1899 to work under von Pechmann and while I was waiting in the laboratory to see the Professor on the opening day of the Semester a young man came up to me and said, "You're English, ar'n't you?" This was Sidgwick. We were given places on the same bench and for the next two years we were working daily side by side.

'Sidgwick had already spent a semester in Germany learning the technique of the new physical chemistry under Bredig in Ostwald's laboratory at Leipzig. [On his journey to Leipzig the little finger of his left hand got infected and became so inflamed that the terminal joint had to be removed. The after-treatment seems to have been rather rough and ready, for he was sent away with the advice, "*Und wenn Sie nicht schlafen konnen, trinken Sie ziemlich viel Bier.*" This mishap was much regretted by Sidgwick for he had a great love of classical music and the loss affected his piano playing.]

'I had already done some work at Cambridge on dibenzoyl-mesitylene and was familiar with organic analysis, so I was allowed to start straight away on my research on coumalic acid. Sidgwick, however, had had little experience in organic chemistry and had to spend a few weeks in doing combustions and other analyses before he could take the *Verbands Examen*, which he had to pass before he could begin making the acetone-dicarboxylic ester and β-iodopropionic ester which were the starting materials for his Doktor-Arbeit.

'We began the day at a quarter past 7 with von Pechmann's lecture on general organic chemistry which lasted till 8 (the "*academische Dreiviertel*"). Pechmann showed a good many experiments and had various dodges. If for example he wished to show the inflammability of a heavy gas like ethyl chloride he would drive it from a glass cylinder by pouring in water from a jug to make it burn freely at the top. He had a lively sense of humour. Speaking of long-chain hydrocarbons he said there was no theoretical limit to their length; in fact one should in theory be able to make a molecule so big "*dass man hätte damit Tennis spielen können*". To illustrate the phloroglucinol test for lignin he applied the reagent first to some good writing paper getting a faint pink colour, then to a reputable Munich paper, the *Schwäbische Merkur*, which gave a distinct reaction, and lastly to the local organ, the *Tübinger Kronik*, which instantly turned the most intense red, provoking a storm of applause.

'After the lecture we went away for breakfast and then came back and worked till the laboratory closed at 6, with of course the usual interval for Mittag-essen.

'We also took lectures in our Neben-fächer, Physics and Geology. The geology included crystallography and mineralogy and from the practical classes we got to know a good deal about crystal forms and learnt to recognise the common minerals.

'The main chemical laboratory was supervised by Carl Bülow who had something of the build and, as we thought, of the mentality of a Prussian drill-sergeant. Before coming to Tübingen he had worked on dyestuffs in the Badische Anilin und Soda Fabrik at Ludwigschafen. He and his students were busily engaged in coupling diazo-compounds with all the β-diketones and β-ketoesters he could think of. It was the kind of work welcomed by the less gifted *Doktoranten* since it yielded an unfailing supply of beautiful crystalline compounds. Bülow was made an *Ausserordentlicher Professor* while we were at Tübingen.

'Edgar Wedekind was also there working as a *Privatdozent*. He had finished with quaternary ammonium salts (one of his first remarks to us was "*Kennen Sie Herrn Po-pe?*") and with a Doktorant was at work on the chloromethyl ether of menthol, which he made by passing hydrogen chloride into a mixture of menthol and formalin. The product was afterwards exploited as a remedy for the common cold.

The most able of the younger men then working in the laboratory was Otto Dimroth, later to become distinguished for his work on triazenes and tautomeric equilibria, and to occupy the chair of chemistry at Würtzburg. While we were at Tübingen he was studying the mercuration of organic compounds by means of mercuric acetate.

'One of the men working under von Pechmann was making pyrazole by combining acetylene with diazomethane. After every fresh brew of diazomethane he had to retire from the laboratory for three or four days to recover from the poisonous effects.

'Von Pechmann usually went round the laboratory every day. He was always smoking a cigar and generally had an old hat on the back of his head. He was very quick. One day I told him I had found that nitrous acid acting on an N-aminopyridone derivative replaced the $.NH_2$ by hydrogen. He at once said, "*Was entwickelt sich dann? Stickoxydul nicht wahr?*" I collected some of the gas over mercury, and the next time he came round the laboratory he tested it himself with a glowing splint.

'He was an exceedingly skilful manipulator and it was a great experience to be taken into his private laboratory and watch him make pilot experiments on a new compound—steam distillation from a test-tube fitted with a cork and bent tube, hot filtration through a cunningly folded piece of filter paper lodged in the mouth of a test-tube, condensation in a test-tube held in a boiling-tube of cold water, and many other contrivances for rapid small scale work—as "small scale" was understood in those days.

'One day the air in the laboratory seemed unusually close and fume-laden. We noticed that large basins of hydrochloric acid and ammonia were being evaporated side by side and that an abnormal amount of heating over sand-baths was going on. The authorities had been urged to provide a new *Chemisches Institut* and the news had been allowed to leak out (we suspected not unintentionally) that an official deputation was coming that day on a visit of inspection. Von Pechmann brought in the party and after he had given them a little time to experience the atmosphere of the laboratory he shepherded them into the combustion room. This was a narrow chamber opening out of the main laboratory with stone benches on either side each with four combustion furnaces and with little space between. Usually about 4 or 5 of the furnaces were in use but on that day all 8 were in full action and the heat was almost insupportable. Von Pechmann kept the party broiling in this Inferno for quite a considerable time while he stood firmly at the entrance and enlarged on the inadequacy of the existing building. We heard afterwards that the provision of a new laboratory had been officially approved.

'Every Saturday afternoon Sidgwick went for a country walk and I took a longer bicycle ride, having at that time trouble with a sprained tendon. On Sunday mornings he used to call for me and we went for a walk, and every Sunday afternoon we had tea together in my room.

'The country about Tübingen was very attractive. It had a rich and varied flora and it was there that my interest in field botany first developed. Sidgwick already had a good knowledge of flowering plants as well as of butterflies and moths and he used to come back with interesting specimens from his Saturday afternoon rambles. I remember him bringing in *Gentiana verna* and *Orchis ustulata*, but his best find was I think *Cephalanthera rubra*, an orchid which in this country is exceedingly rare, occurring only in one Cotswold wood.

'On the first Whitsuntide holiday we went together to Switzerland where we spent about ten days. Our longest stay was at Grindewald whence we went to the Jungfraujoch and to Wengern Alp where we saw a wonderful array of alpine flowers. The following Whitsun Sidgwick went to Oxford to be admitted to his Lincoln fellowship and I went to Switzerland alone. Sidgwick told me that on his journey down the Rhine valley the railway cuttings were yellow with the flowers of *Isatis tinctoria* (woad) but when he returned a fortnight later they were black with the same plant in fruit.

'We spent a week-end in Stuttgart to hear performances of the parts of *Der Ring des Nibelungen* at the Opera House and I think it must have been on this visit that we went to the Stuttgart *Tiergarten* and noticed while we were looking at the animals that it was becoming surprisingly dark. We then realised that an extensive partial eclipse of the sun was in progress, and were interested to observe in the tree shadows the crescent-shaped images of the partially blacked out sun (pin-hole camera effect).

'We finished our dissertations and had our oral examinations in July 1901, Sidgwick taking his 10 days or so before I had mine. We had as examiners von Pechmann, Paschen (physics) and Koken (geology). I think we both found the questions rather searching.

'Last August I had a very pleasant surprise. I got a large cardboard cylinder from the Dean of the Faculty of Science of the University of Tübingen. It contained four printed copies of a Diploma, one of them sealed with the University seal and signed by the Rektor and the Dean of Faculty. It was just 50 years since I had received my Tübingen degree and this diploma was an *Erneuerung* of the title of *Doktor der Naturwissenschaften* sent "*in dankbarer Anerkennung und mit herzlichen Glückwünschen*".

'Sidgwick will have received a similar document and it will be of interest as it will contain a brief outline of his contributions to chemistry.

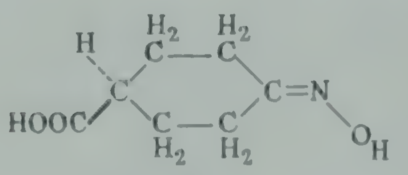
'For both of us our student days at Tübingen were a happy and a profitable time and for me they brought the privilege of close companionship with Sidgwick. Our outlook and our interests had very much in common and the friendship then begun lasted through life. The happy chance that had thrown us together I shall never cease to regard as my great good fortune.'

The London period 1902-1912

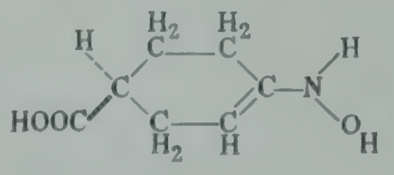
I think it is probable that most chemists who knew Mills only in his later Cambridge period must find it difficult to visualize him working happily under the conditions which were normal at most technical institutes at the beginning of the century. Certainly, Mills never mentioned this period to me, and I was working in the same department with him in Cambridge from 1920 until he retired, and saw him periodically afterwards. These technical institutes were usually financed largely by municipal authorities, who often took the simple view that if a man was engaged as a lecturer, he 'lectured'

—in lecture room or laboratory—throughout the official hours during which the institute was open, precisely as a clerk would pursue clerical work in the office. In certain institutes even the head of a department could not be absent for a day without permission, which was normally granted only for a reason directly connected with his work, such as attendance at an important committee. Consequently, very little time was available for research, and the facilities, in bench space, apparatus and chemicals, were almost non-existent. These conditions have now changed so thoroughly that the rising generation of chemists can hardly visualize them and probably would be unable to survive them. Nevertheless, in a few centres throughout the country a small number of very keen, hardworking chemists did carry out work of high quality in those days, often by working throughout the week-ends.

Mills himself contrived to pursue some research with other members of the staff, particularly Mr W. H. Watson and Miss A. M. Bain, and also with Miss S. T. Widdows of the Royal Free Hospital Medical School. In particular, he started his long line of stereochemical researches. The Hantzsch–Werner theory of the isomerism of the oximes provided an excellent explanation of this isomerism, but in the absence of any decisive proof of its accuracy many chemists still considered that the isomerism was based on structural differences. Mills sought some means, based on optical activity, of providing this proof, and in collaboration with Miss A. M. Bain synthesized 4-oximino-cyclohexanecarboxylic acid (I).* Clearly, this compound will



(I)



(II)

possess a plane of symmetry if the $=C=N\cdot OH$ group is linear: if, however, the $\cdot OH$ portion of this group is bent down whilst remaining in the general plane of the ring, as in (I), the compound is dissymmetric and should be capable of resolution into optically active forms. (In formulae (I) and (II), the valencies are regarded as being in the plane of the paper, except those in thick and in dotted lines, which project respectively towards, and away from, the observer.) This resolution was successfully accomplished (although the compound not unexpectedly underwent rather rapid racemization in solution) and strong evidence for the Hantzsch–Werner theory was thus obtained.

In 1903 Mills had married Miss Mildred Gostling whom he had met when she was working, as Bathurst Student of Newnham College, with

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* Throughout this notice, I have in genenal adhered to the nomenclature for organic compounds used by Mills, in order to facilitate reference to the Bibliography. Certain names therefore are not in accord with modern nomenclature.

H. J. H. Fenton on bromomethyl-furfural. It was during this London period that he also published research, in collaboration with his wife, on dinaphanthracene.

The Cambridge period 1912-1938

When Mills returned to Cambridge, Dewar had to all intents and purposes already left Cambridge for London, although he retained his Cambridge Professorship until he died. In view of his prolonged absence, the University had appointed Mills to act as his deputy (on a very modest stipend), and in this capacity Mills very conscientiously gave an excellent course of lectures on low-temperature chemistry. This subject could not, however, have been of great interest to him, nor could the conditions have been congenial, for Dewar on his very rare and brief visits to the University appeared to believe that on principle he should utter adverse criticism of his deputy's performance, of which he must have been in fact almost wholly ignorant. Certainly the lectures ceased immediately Dewar's death released Mills from this office.

Meanwhile, however, he had been developing his organic research work, but the onset of the 1914 war gave this an entirely new direction, namely, the investigation and production of cyanine dyes capable of sensitizing photographic plates. This work, in which he initially collaborated with Pope and later with a number of research students, occupied him throughout the war and for several years afterwards, and resulted in a great extension and clarification of our knowledge of these fascinating compounds. Immediately after the war, however, he also resumed his stereochemical studies which he continued long after his cyanine work had finished. The greater bulk of Mills's research was on these two subjects; the stereochemical work, having both a wider scientific range and a greater volume, will be discussed first.

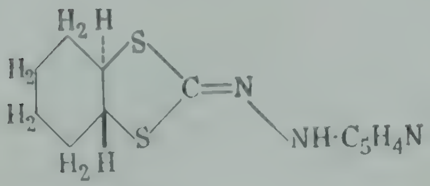
Stereochemical studies

Mills's work covered several distinct types of stereochemical studies, some of which were continued for several years. It is better therefore to give an outline of his results with each type of study in turn, rather than deal with all his stereochemical work in chronological order.

Isomerism of the oximes

When Mills and Bain resolved the oxime (I), they were fully aware of the possibility that this compound might have undergone conversion into the isomer (II), or even form a tautomeric mixture of the two compounds in solution. If, however, any conversion into the isomer (II) had occurred, the resolution lost its significance, for the compound (II) possesses an asymmetric carbon atom. Mills adduced considerable evidence that their compound had in fact the structure (I), but an element of doubt remained.

He therefore sought a similar type of compound in which this isomerization could not occur, and in 1923 published with H. Schindler an account of the synthesis and resolution of the pyridylhydrazone of *cyclo*hexylene-dithiolcarbonate (III). This appeared to provide the decisive confirmation of the Hantzsch–Werner theory. The stereochemistry of hydrogenated fused ring systems was not apparent in 1923, however, but Mills later realized that if the two angular hydrogen atoms in his compound (III) were in the *trans* position to one another, the compound was dissymmetric even if the $:C{=}N{-}NH{-}$ portion were linear. The final and undoubted proof came in 1931, when he and B. C. Saunders prepared and resolved the *o*-carboxy-phenylhydrazone of β-methyl-trimethylene-dithiolcarbonate (IV), a compound which possessed great optical stability. It should be added that all subsequent work, of a wide variety and nature, has confirmed the truth of the Hantzsch–Werner theory, which Mills was thus the first to establish conclusively.



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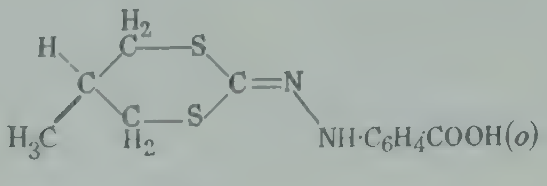
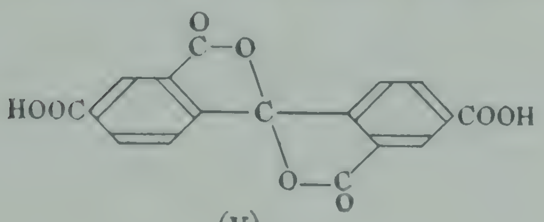


(IV)

Spirocyclic work

It had long been recognized that a spirocyclic compound in which a carbon atom was common to two rings, which themselves had suitable substituents, should be dissymmetric and therefore susceptible to optical resolution. Mills published in 1915 a brief preliminary note on the synthesis of the ketodilactone of benzophenone-2,4,2′,4′-tetracarboxylic acid (V), a compound which fulfilled the necessary conditions, and had acidic groups



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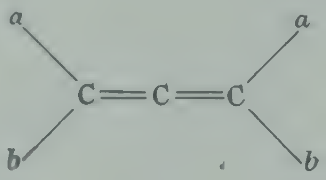

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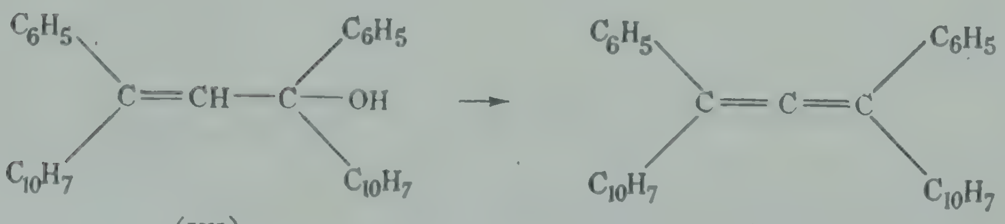
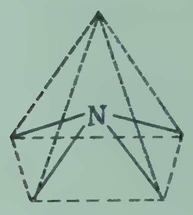


(VII) (VIII)

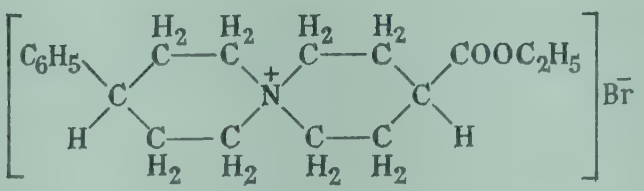
for salt formation with optically active amines. He and C. R. Nodder combined this acid with several optically active alkaloids but fractional crystallization of the salts gave no evidence of resolution. Mills insisted on the use of alkaloids, and I vividly remember the occasion when he left for a few days to examine in the Final Honours School at Oxford, and Nodder hastily seized his opportunity, prepared and resolved some α-phenylethylamine, combined it with the acid (V), recrystallized the salt, and obtained decisive evidence of resolution to justify his action before Mills returned.

The allenes represent another class of compound which possess essentially the simplest type of spirocyclic structure, and van't Hoff had recognized some fifty years earlier that a simple allene derivative of type (VI) should be capable of existing in optically active forms. All attempts to prepare and resolve these simple allene derivatives had hitherto failed, however. In 1935, P. Maitland and Mills, after about 6 years of persistent and initially very discouraging work, were able to announce the isolation of the two optically active forms of αγ-diphenyl-αγ-di-α-naphthyl allene (VIII), which they obtained by the stereospecific dehydration of the corresponding allyl alcohol (VII), using *dextro* and *laevo* camphorsulphonic acid.

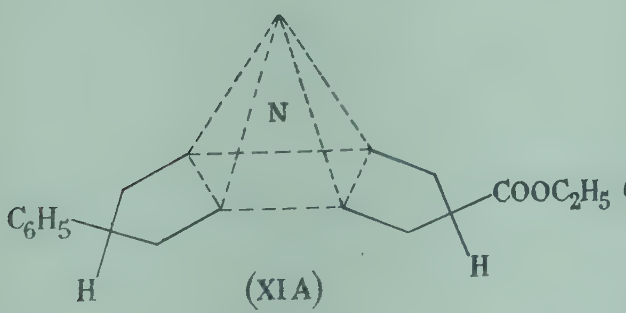
Mills also utilized a spirocyclic salt to obtain evidence for the configuration of the nitrogen atom in quaternary ammonium salts. Bischoff had considered that this atom could be regarded as situated in a pyramid, with its four valencies directed downwards to the apices of the square base (IX). The other possible arrangement of the valencies would be tetrahedral as in carbon. To decide this point, Mills and L. Bains in 1925 synthesized 4-phenyl-4′-carbethoxy-*bis*piperidinium-1:1′-spirane bromide (X). If the nitrogen atom in this compound had the Bischoff pyramidal configuration, the cation should be capable of existing in *cis* (XIA) and *trans* (XIB) forms, both of which have a plane of symmetry. If, however, the nitrogen atom is tetrahedral, the cation is dissymmetric, but cannot show geometric isomerism.



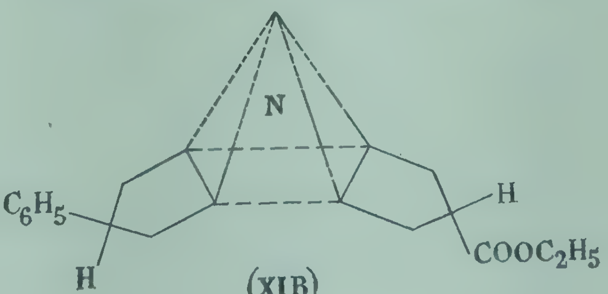
(IX)



(X)



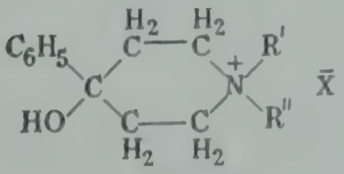
(XIA)



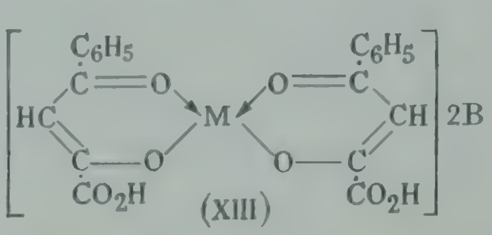
(XIB)

The optical resolution of this salt, published with E. H. Warren also in 1925, establishes firmly the tetrahedral configuration for the ammonium ion in this salt.

Mills must have realized that there was some danger in basing a generalization regarding the configuration of the ammonium ion solely on the properties of one rather specialized type of cation, for he discussed at some length the relative dimensions of the units of the piperidine ring in order to show that the four valencies of the central nitrogen atom would almost certainly not be strained from their normal positions by incorporation in the spirane system. To obtain additional evidence without recourse to spirane compounds, he prepared, with J. D. Parkin and W. J. V. Ward, a number of 4-hydroxy-4-phenyl-piperidinium salts of type (XII). If the quaternary nitrogen atom had the pyramidal configuration, these salts should exist in *cis* and *trans* forms even if the groups *R′* and *R″* were identical, and moreover if these groups were different each form should be susceptible to optical resolution. On the other hand, if the nitrogen had the tetrahedral configuration, *cis* and *trans* forms would occur only when *R′* and *R″* were different, and neither form would be resolvable. Five salts of type (XII) were prepared: the three salts each having different *R′* and *R″* groups were separated into geometric isomers, whereas the two salts in which *R′* and *R″* were identical showed no indication of this isomerism. This evidence is to some extent negative in quality, but it is in full accord with the tetrahedral configuration.



(XII)

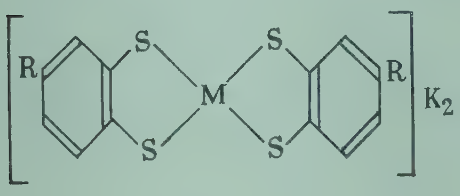


(XIII)

This work, apart from the great value of its prime object, served a subsidiary purpose in reminding chemists of the mere possibility of the Bischoff configuration, and of the fact that the optical resolution of a simple compound of type *Eabcd* does not *prove* that the element *E* has the tetrahedral configuration, an assumption, however, which is still occasionally made (e.g., N. V. Sidgwick, *The chemical elements and their compounds*, 1950, **1**, 801).

Mills used chelated metallic derivatives, essentially of the spirane type, to investigate the stereochemistry of various metals. With R. A. Gotts in 1926, he co-ordinated benzoylpyruvic acid, $C_6H_5COCH_2COCOOH$, with beryllium, cupric copper, and zinc to obtain, with various alkaloids, salts of type (XIII) where *M* represents the metal and *B* the alkaloidal base. The brucine salt of the beryllium compound was obtained in two forms, which in solution underwent rapid mutarotation to give the same rotatory value. One form, when rapidly treated in alcoholic solution with dimethylamine hydrochloride, deposited the brucine almost quantitatively as brucine hydrochloride, and the filtered solution of the dimethylamine salt of the acid (XIII; M=Be) then underwent rapid and almost complete racemization. These results showed that the 4-co-ordinated beryllium atom must have the tetrahedral, and not the planar, configuration. The strychnine salt of the copper derivative (XIII; M=Cu) and the brucine salt of the zinc derivative (XIII; M=Zn) also showed the mutarotation in solution. There is now, however, much independent evidence that the 4-co-ordinate cupric atom is planar whereas the zinc atom is tetrahedral, and the significance of Mills and Gotts' observations on their cupric compound has never been satisfactorily explained.

Mills and R. E. D. Clark investigated the mercury, cadmium, and zinc



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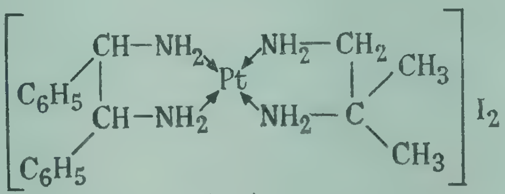


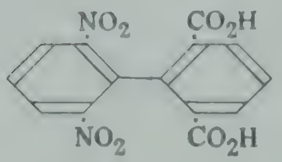
(XV)

complex salts of toluene-3,4-dithiol (XIV; $R=CH_3$) and of 1-chlorobenzene -3,4-dithiol (XIV; R=Cl). These three 4-co-ordinate metals have the tetrahedral configuration, and their salts of type (XIV) should therefore be resolvable. The salts obtained with optically active bases were each separated into two forms, presumably diastereoisomers of very low optical stability, and replacement of the active base by an inactive base or cation always gave an inactive salt.

Mills adopted a slightly different approach in an attempt on these lines to determine whether the 4-co-ordinate platinous atom had the planar or tetrahedral configuration. For this purpose, he and T. H. H. Quibell co-ordinated *meso*-1,2-diphenylethylenediamine and 1,1-dimethylethylenediamine to platinium to give the salt (XV). If the four valencies of the platinum atom are planar, the cation of this salt is dissymmetric, whereas if the valencies are tetrahedrally arranged the cation has a plane of symmetry. The optical resolution of this compound gave decisive evidence for the planar configuration of the platinum atom, and it was followed in 1939 by the resolution of the analogous palladium compound (with A. G. Lidstone). At the time of publication of these results, there was already other stereochemical and crystallographic evidence for the configuration of each metal, but Mills's results afforded a very elegant and entirely independent proof.

Restricted rotation

Mills's keen mind provided the explanation of the fundamental factor which underlay an entirely new branch of stereochemistry, for which the first evidence was the optical resolution of 6,6′-dinitrodiphenic acid (XVI) by Kenner and Stubbings in 1921. On the basis of classical stereochemistry, however, the acid (XVI) should have a plane of symmetry by virtue of the free rotation of the benzene rings about the bond connecting them. Kenner subsequently resolved several 6-substituted and 6,6′-disubstituted diphenic acids, and several suggestions were put forward to explain the cause of the optical activity. In 1926 Mills, in a letter published in *Chemistry and Industry*, pointed out with the aid of a simple diagram that an assessment of the comparative sizes and positions of the atoms in these molecules showed that the *ortho* substituents were too large to allow this free rotation, and that the restricted rotation caused by their mutual obstruction prevented the molecule assuming a planar configuration: the molecule was therefore dissymmetric. This explanation, which to the younger generation of chemists may now seem so obvious, immediately clarified the problem and was rapidly and universally accepted.



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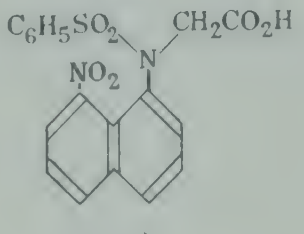
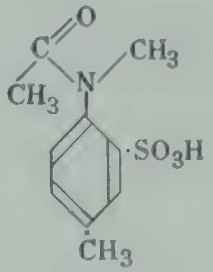


(XVII)



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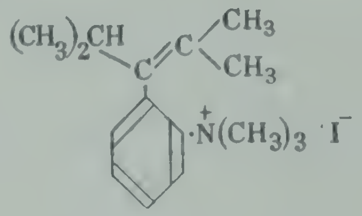

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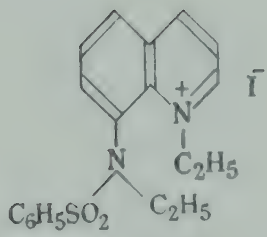


(XVIII)

Mills became greatly interested in the stereochemical aspects of restricted rotation, but left the study of substituted diphenic acids to be developed largely by Turner in this country and by Roger Adams in America. Mills turned his attention to other systems, and demonstrated the optical activity of benzenesulphonyl-(8-nitro-1-naphthyl)glycine (XVII) with K. A. C. Elliott in 1928, and of 8(benzenesulphonyl-ethylamino)-1-ethylquinolinium iodide (XVIII) with J. G. Breckenridge in 1932: in these compounds rotation about the bond joining the sulphonamido nitrogen atom to the aromatic nucleus is restricted by the bulk and proximity of the nitro group in the compound (XVII) and by the quinolinium ethyl group in the compound

(XVIII). Both these compounds underwent rather rapid racemization in cold solutions, indicating that in each case complete rotation, although hampered, was not entirely suppressed.

Mills then investigated the possibility of realizing optical activity due to restricted rotation in a simple substituted benzene derivative, i.e., one in which the benzene ring did not have another ring either fused or directly linked to it. After a careful assessment of the nature and position of the substituents necessary to achieve this purpose, he synthesized and resolved *N*-acetyl-*N*-methyl-*p*-toluidine-3-sulphonic acid (XIX) with R. M. Kelham in 1937, and *o*-($\beta\beta$-dimethyl-α-isopropylvinyl)phenyl-trimethyl-ammonium iodide (XX) with G. H. Dazeley in 1939. Optically active salts of the acid (XIX), for example, the sodium salt, underwent racemization in solution at room temperature in a few hours, but the iodide (XX) possessed great optical stability, and its rotatory power was unaffected when the salt in aqueous solution was boiled for eight hours.

In another branch of stereochemical studies, Mills and R. Raper in 1925 were able to resolve *p*-carboxyphenyl-ethylmethylarsine sulphide, $HOOC \cdot C_6H_4 \cdot As(CH_3)(C_2H_5)S$, into optically active forms. This confirms (but of course does not prove) that the arsenic atom in tertiary arsine sulphides has the tetrahedral configuration. It is noteworthy that no comparable tertiary arsine oxide has yet been resolved, although analogous tertiary phosphine oxides and sulphides have been obtained in optically active forms; furthermore, quaternary phosphonium and arsonium salts, and spirocyclic phosphonium and arsonium salts have also been resolved. All these six types, with the exception of the tertiary phosphine oxides, were resolved in the Cambridge laboratories by Mann and his co-workers.

At the British Association meeting in 1932, Mills was President of Section B, and delivered his Presidential Address on 'Some aspects of stereochemistry'. In this lecture he put forward a mechanism for the Walden Inversion which has since been amplified but not fundamentally changed. His mechanism for the Beckmann oxime rearrangement, although put forward in a very simple way, also had a direct influence on more modern theories. Mills's theories on these subjects have not received adequate recognition, possibly because their publication in the *British Assocation Reports* escaped general notice.

Cyanine dyes

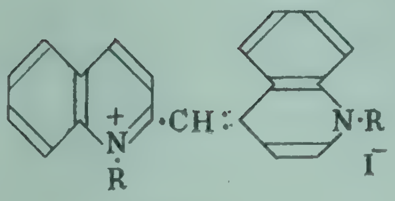
When during the First World War the Western Front became established essentially as two parallel bands of heavily entrenched positions, aerial reconnaissance by the Royal Flying Corps was conducted under a heavy disadvantage. A photographic plate having a silver bromide-silver iodide emulsion is sensitive only to light within 360 to 490 mμ, i.e., to the ultraviolet, violet and blue regions. Under conditions of trench warfare, it became imperative to detect from the air each day as soon as the increasing light allowed, any operations which the enemy might have carried out in his trench system and elsewhere during the previous night. The bromide-iodide plates, however, were at their least sensitive in the red sky of the early morning. The German aeroplanes did not suffer from this defect, for the German chemical firms had prepared certain 'photographic sensitizers' which when incorporated at great dilution in the emulsion of the plate extended its sensitivity well into the red region. The preparation of one of the best of these compounds, named pinacyanol, was patented in Germany as far back as 1905, and it is curious that the German firms had not in the meantime developed this valuable field with greater vigour.

Pope was asked to investigate these compounds and of course to prepare supplies as rapidly as possible, a task in which Mills fully collaborated. Mills and Pope, writing in the *Photographic Journal* in 1920, summarized thus the position, and the outcome of their efforts. 'As it was clear that panchromatic photography would develop greatly in the Air Service as an instrument of warfare, and as the importation of sensitizing dyestuffs would soon cease, we commenced the study of the more necessary of these latter substances and devised methods for their preparation on a sufficiently ample scale. Throughout the war practically all the sensitizing dyestuffs used by the Allies in the manufacture of panchromatic plates were produced in this (i.e. the *Cambridge*) Laboratory and, as the air photography for reconnaissance purposes extended, the ordinary blue-sensitive plate became more and more completely replaced by the panchromatic plate.'

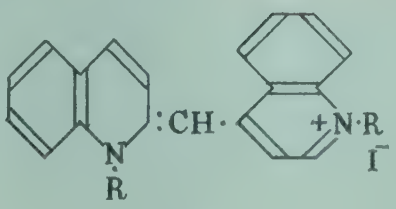
The interest of the cyanine dyes is emphatically not limited to their technical application in photography: the mechanism of their formation, and the essential factors in their structure involve theoretical considerations of very great interest. There are, however, very many different types of cyanine dyes, and a reasonably brief yet lucid summary of the work by Pope, Mills and their co-workers is almost impossible to write. Only an outline of some of their results, published mainly in the period 1920-1926, is therefore given.

In 1920, Pope and Mills published two papers recording the preparation, optical absorption and sensitizing action of a considerable range of cyanine dyes, all of which were members of either the *iso*cyanine or the carbocyanine class, and they postulated the correct structure for the carbocyanines: pinacyanol, which is a valuable sensitizer for red light, is a member of this class. In the same year, Mills and Wishart showed that the '*iso*cyanines' had 'one of the (supposedly) tautomeric formulae' (XXIA) and (XXIB), but that their evidence did not 'provide a satisfactory basis for distinguishing between these two'. It was not, of course, clear at that time that the two cations depicted in the formulae are canonical forms of a common hybrid.

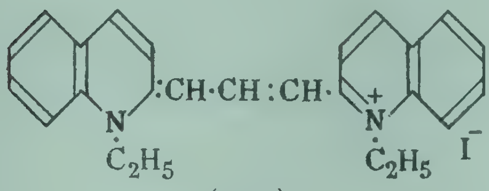
Later in 1920, Mills and Hamer showed decisively that pinacyanol had in fact the postulated structure (XXII) and that it was thus a member of the 2,2′-carbocyanine class: on the basis of this work, they predicted the existence of the analogous classes of cyanine dyes, the 2,4′- and the 4,4′-carbocyanines. The essential general structure of cyanines had by now emerged, namely, that the compound must possess two nitrogen atoms linked through



(XXIA)

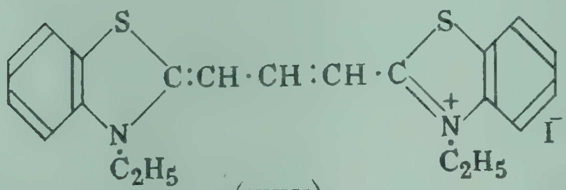


(XXIB)



(XXII)

an odd number of carbon atoms conjugated in such a way that either of the nitrogen atoms could accept the positive charge.



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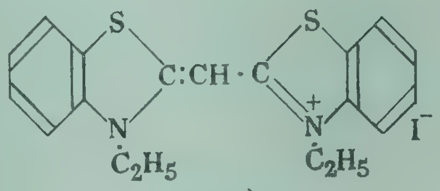

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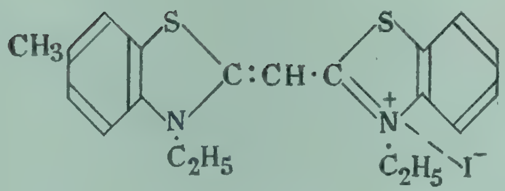
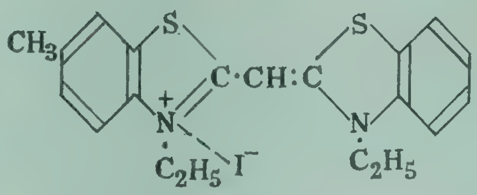


(XXVA)

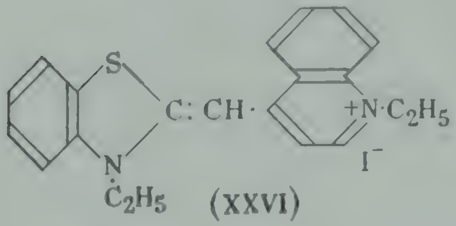


(XXVB)

In 1922 Mills published his work on the interaction of benzothiazole ethiodide and 2-methylbenzothiazole ethiodide in various media. He showed that these salts interacted to form two products. The major product was a purple dye, which was a powerful sensitizer for the green, yellow and orange regions, and which had the structure (XXIII): he remarked that 'it is the pinacyanol of the benzothiazole series' and proposed the name carbothiocyanine for this class of compound. The minor product was a yellow dye which was a sensitizer 'for blue rays'. He showed conclusively, by an independent synthesis, that it had the structure (XXIV), and was a member of a new class which he termed thiocyanines.

Mills utilized the benzothiazole system to synthesize with W. T. K. Braunholtz by unambiguous routes the two apparently isomeric salts

(XXVA) and (XXVB) (reproduced here as they were depicted in the original paper), and found that they were identical, and thus existed in a condition of 'virtual tautomerism'. Even so, it was still too early in the development of structural theory to divine the fundamental property, namely, that the two cations were not tautomeric but identical. They write: 'Which of the formulae [i.e. (XXVA) or (XXVB)] is to be assigned to the crystalline product to which both of the reactions give rise has not been determined. It will evidently be the salt derived from the stronger of the two corresponding bases and therefore on account of the well-known influence of methyl groups on the strength of bases, the constitution [(XXVB)] must be regarded as the more probable.'



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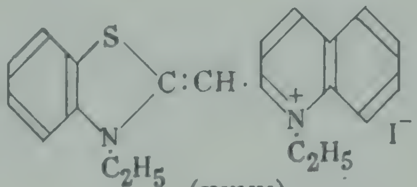

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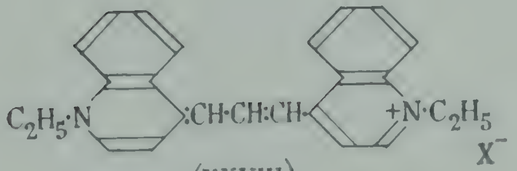

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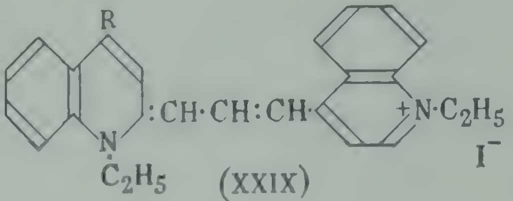


(XXIX)

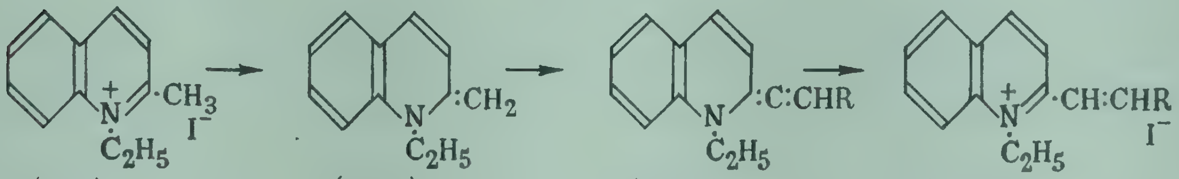
Mills and Braunholtz also synthesized the red thio*iso*cyanines of type (XXVI) and the orange thio*pseudo*cyanines (XXVII): they showed, moreover, that the blue kyptocyanine, which had previously been prepared by Adams and Haller, and which was a valuable sensitizer for infra-red light, was in fact the predicted 4,4′-carbocyanine (XXVIII), isomeric with the compound (XXII).

In collaboration with R. E. Odams, Mills synthesized the 2,4′-carbocyanine (XXIX; $R = H$), and showed conclusively that the 'dicyanine', which had been prepared by Adams and his co-workers, and had been found to be a sensitizer for infra-red photography, was the homologue (XXIX; $R = CH_3$). In 1928, with H. G. Ordish, Mills published the last of his cyanine papers, on the constitution of the apocyanines.

The above summary of the elucidation of the main types of cyanine dyes is too brief to do justice to Mills's work: furthermore, it gives no indication of the considerable volume of auxiliary work which Mills and his co-workers carried out on the preparation and properties of many compounds, such as benzothiazole derivatives, which were either intermediates in the syntheses of the final products or which alternatively had chemical properties significantly allied to those of the final products.

One point of considerable general interest arose in this cyanine work. The cyanine syntheses frequently involved the condensation of a quaternary salt of a heterocyclic nitrogen compound in which the heterocyclic ring had a 'reactive' methyl substituent, reactive in the sense that with suitable aldehydes and nitroso compounds it would undergo a condensation, which, for example, the methyl group in toluene does not undergo. This high reactivity was, however, shown only when the methyl group was in certain positions, such as the 2 and 4 positions in the quinoline ring and the 1 position in the *iso*quinoline ring, and it was considerably enhanced by the presence of very small quantities of other reagents. These facts had been known, and the underlying factor even tentatively suggested, long before Mills's work, but no investigation sufficiently thorough and skilful to give a convincing explanation had been made.

Mills discussed this subject in 1922 in a paper with J. L. B. Smith (later to become Professor of Ichthyology at Rhodes University, Grahamstown and to be the first man to identify and make a scientific examination of the coelocanth), and again in great detail in 1925 with R. Raper. He emphasized that, for example, quinaldine will condense with *p*-dimethylamino-

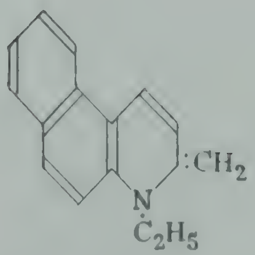


(XXX) (XXXI) (XXXII) (XXXIII)

benzaldehyde if the mixture is heated at 100 °C with zinc chloride for some hours, but that the ethiodide of quinaldine (XXX) will rapidly condense with the aldehyde even in dilute alcoholic solution if a trace of piperidine is present. Mills therefore considered that the action of the piperidine on the ethiodide (XXX) was to form the anhydro (or methylene) base (XXXI), and that the latter was the essential reactive intermediate, as Vongerichten and Höfchen had suggested on other grounds in 1908.

The highly reactive methylene base would then condense with an aldehyde to give a product of type (XXXII) having essentially an allene structure: this compound would react rapidly with hydrogen iodide derived from the piperidine hydriodide, and thus give the salt (XXXIII) and liberate free piperidine which could then repeat the cycle.

Mills attempted to confirm this sequence of reactions by isolating the methylene base (XXXI) and showing that it did in fact condense readily with aldehydes to give products which in turn with acids gave cyanine dyes. The instability of the base (XXXI) made these experiments unsatisfactory, but the corresponding 5,6-dibenzo compound (XXXIV), a yellow crystalline compound, combined readily with *p*-dimethylaminobenzaldehyde in



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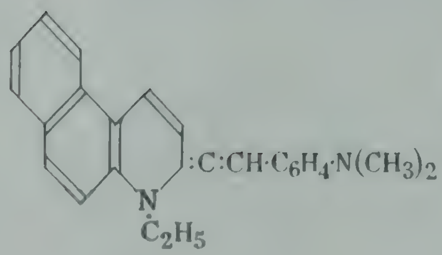

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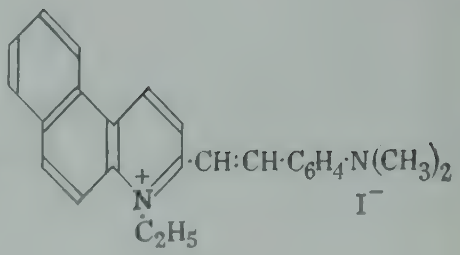

(XXXVI)

the absence of a 'catalyst' to give the product (XXXV), which in turn with hydriodic acid gave the cyanine dye (XXXVI).

These experiments, combined with the fact that valency considerations indicate that methylene bases of the type (XXXI) and (XXXIV) could be formed only from the quaternary salts of 2- and 4-methylquinoline and 1-methyl*iso*quinoline, gained general acceptance for this mechanism. During recent years some doubt regarding the intervention of the anhydro(methylene) bases in all such condensations has been expressed by Brooker and White, and by other chemists, but the condensations which do not involve these bases must still be regarded as exceptional.

The chemistry of cyanine dyes has now opened up into an immense field requiring highly specialized study. One of Mills's early collaborators, Dr Frances M. Hamer, has continued working vigorously in this field and is recognized as one of the leading authorities on this branch of organic chemistry.

During the period between 1912 and his retirement in 1938, Mills continued working in Cambridge with only one break, when he went to Cornell University in 1937 to give the George Fisher Baker Lectures. He lectured on stereochemistry, and many chemists then looked forward eagerly to the printed work, for one condition of appointment to the Lectureship is that the lectures shall subsequently be published. In fact, however, he never prepared them for publication. On one occasion a few years afterwards I asked him when we should have the great pleasure of seeing this volume, and he replied, 'Never. I'm much too lazy to write up all that stuff.' Certainly he was slow to write up even his own work unless there were active competitors in the field, and a general volume would make far less appeal to him. My own impression, however, was that he was very sensitive to criticism, and therefore restricted his scientific writings almost wholly to his own research work, upon which he could be certain of the facts and of their most reasonable explanation: he hesitated (I feel) to write on the wider field which would necessarily contain various branches of which he was not a complete master.

In 1942 he delivered the Pedler Lecture of the Chemical Society on 'The basis of stereochemistry'. He was President of the Chemical Society for the periods 1942-1943, and 1943-1944, and his presidential addresses were on

'The stereochemistry of labile compounds' and 'Old and new views on some chemical problems', and these addresses form his last chemical publications, other than brief historical articles.

Whilst President of the Chemical Society he had on the Council by a remarkable chance both the Senior and the Junior Proctor of Cambridge University (Dr B. C. Saunders and Dr G. B. B. M. Sutherland respectively). The chance that two scientists should be Proctors simultaneously was small, that they should both be chemists much smaller, and that they should be elected together to the Council almost infinitesimal.

Mills was elected to the Fellowship of the Royal Society in 1923, and received the Davy Medal in 1935. The Chemical Society awarded him the Longstaff Medal in 1930.

The quality of Mills's work on stereochemistry or on cyanine dyes would —on either subject alone—have placed him in the top rank of organic chemists: the two branches together form a brilliant and massive whole. Certainly his stereochemical work, in its wide scope, skilful deployment and great scientific value, reaches a level which no other stereochemist has attained.

Many chemists were puzzled by the fact that although the Cambridge organic laboratories between the two world wars had two such brilliant men as Pope and Mills, the number of research men tended steadily to decline throughout this period. Certainly Pope, who always stimulated and encouraged research, found that external calls on his time—from Government, legal and technical sources—became steadily more insistent and more arduous during this period, towards the end of which he suffered increasing ill-health. This mounting preoccupation with external work, however, caused him to hand more and more research students to Mills, who should have had an increasingly flourishing school of research. The various reasons for the decline cannot be happily expressed. Mills undoubtedly excited the deep loyalty of his students, but he also taxed it heavily. He had a curious habit with new research students of suggesting that if a certain compound could be prepared, it 'would be interesting', but the student might then work for a year or more on the synthesis before he received any hint regarding the subsequent interesting application of the compound which Mills had in mind.* Mills also expected his research men both to order and buy any chemicals they required which were not available in the very limited departmental store. This was a severe hardship on students who were usually subsisting on very small grants, and often adversely affected their work: an able student who found that one stage in a proposed synthesis was in fact impracticable might think of various other more promising routes, but be unable to buy the reagents which these routes required. This system pressed with great severity on men in their third and normally most productive year of research, because over much of this period the grants provided by the Department of Scientific and Industrial Research covered only two years: consequently men in their third year (which the Cambridge Ph.D degree regulations required) often had to subsist entirely on money earned by demonstrating, coaching and other casual employment. (Mills's attitude on this matter was surprisingly different from that of Pope. When I was a young man working on the co-ordination derivatives of aliphatic polyamines with various noble metals, and went to Pope at intervals for supplies of these metals, he would unlock his desk and hand me lengths of tubular platinum, gold coins of foreign denomination, and large bottles of sodium rhodium chloride.) More particularly, however, when Mills was asked for an opinion on even an able and experienced student, he would write in so subdued a style that a committee would draw an entirely incorrect inference regarding the student's ability and promise. Pope was always deploring the small number of organic chemists having College Fellowships, but the Colleges, when seeking promising chemists, usually went to Mills, with whom they were better acquainted, for an opinion. Two organic chemists were elected to College Fellowships during this long period: neither was working with Mills.

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* It is possible that Mills owed this curious habit to his early training under von Pechmann. In a covering note to the article on 'Tübingen 1899-1901', he remarks: 'When von Pechmann suggested the problem to him (Sidgwick), he probably saw pretty clearly how it would turn out, but he revealed nothing of this to Sidgwick, who was directed to carry out the successive stages and had to try to interpret them as he observed them.'

Mills was devoted to his College, of which he was President (i.e., Vice-master) from 1940-1948, and indeed he was deeply versed in the history and the lore of the University in general and the Colleges in particular. He had a fund of anecdotes, mainly about past members of the University, and when —particularly in the relaxation of the Combination Room after Hall, with chemistry temporarily thrust aside—he told some of these stories, always with excellent delivery and without the faintest malice, the deep enjoyment depicted in his face revealed unforgettably the warm, human side of his personality.

When he retired and became Emeritus Reader, he dropped all experimental work, and with the exception of his engagements at the Chemical Society, devoted almost all his time to the study of natural history in the field. These studies had occupied his leisure hours throughout his working life, and he was already an authority in particular on the birds and the plants of East Anglia. His retirement gave him an opportunity to devote himself to his prime interest in this field, namely, the many sub-species of British bramble. These he collected, classified, and finally preserved in the Botany Department of the University. I am greatly indebted to Dr S. M. Walters, the Curator of the University Herbarium, and to Mr P. Sell, who worked very closely with Mills in the last few years of his life, for the following account of his botanical work.

'The W. H. Mills collection of *Rubi* is composed of about 2400 sheets, of which 2200 are mounted and arranged in systematic order, and 200, on which he was working at the time of his death, are still between newspaper. His selection of specimens was very carefully made, his pressing was always done in the field, and his mounting was meticulous (showing in detail all the important characters to their best advantage). The result is that his specimens are almost perfect. He travelled the country a good deal and had representative gatherings of 320 of the 389 'micro-species'' of *Rubus fruticosus* at present recognized as occurring in the British Isles. He published only one paper on the subject, *Rubus watsonii*, sp. nov., in *Watsonia*, **1**, 135 (1949). The specific epithet was after W. C. R. Watson, his friend and fellow batologist, for whom he had great regard. He disagreed with him, however, in that he believed the species from the Cornish Peninsula described by F. Rilstone were distinct, and not identical with more widespread species as stated by Watson. These species are kept in separate folders in his herbarium. There is no doubt that any serious batologist making a study of British species would have to see the Mills herbarium. Not only are the specimens ideal but many have been confirmed by Watson. During the last few years of his life Mills identified many of the specimens in the University Herbarium, and he was working on these only a few hours before he died.

'His botanical interests were not confined to *Rubus*, for he knew the flora of Cambridgeshire and the adjoining counties as well as any other local botanist. He had a strong objection to collecting anything other than *Rubus*, of which he said a little pruning only made the bush grow better. He had seen the great majority of the local rarities and mentioned his finds only to a very few. He found *Crassula tillaea* (*Tillaea muscosa*) new to Cambridgeshire and supplied the lists of *Rosa* (another critical group in which he was interested, but did not collect) and *Rubus* for the additions and corrections to the Cambridgeshire flora published in *Proc. Bot. Soc. Brit. Isles*, **1**, 471 (1955) and **3**, 165 (1959).'

The Report for 1958-59 of the Wicken Fen Local Committee of the National Trust contains the following passage. 'In February the Committee suffered a sad loss in the death of one of its oldest and most diligent members, Dr W. H. Mills. Dr Mills, who had served on the Executive Committee for more than 20 years, was Treasurer during the difficult war years and Chairman from 1947 to 1952, when very important management decisions were made. His knowledge of the Fen, particularly its botany, was unrivalled, and he retained to the last a very lively interest in all Wicken affairs.'

One of Mills's last communications, published as a short article in the *Cambridge Review* in 1955, well illustrates his interest in the history both of matters chemical and biological. The article concerned the chemical laboratory which Bentley, the Master of Trinity, provided in the College for Vigani, who had just (1703) been elected the first Professor of Chemistry in Cambridge, an office which carried, however, neither stipend nor any facilities for teaching. Stephen Hales, the great physiologist, went to the laboratory primarily to see Vigani perform chemical experiments, although

Hales, writing many years afterwards, stated that he also helped to perform some simple experiments concerning the distillation of mercury. Mills traces the course of subsequent references to Hales's attendance at this laboratory and shows how, stage by stage, the chemical aspect is suppressed and ultimately replaced by a spurious physiological aspect, until Alexander Wood, writing in 1954, states decisively 'In a sense the first physiological laboratory was that provided by Richard Bentley . . . Here Stephen Hales first measured blood-pressure, and worked at animal and plant physiology before becoming perpetual curate at Teddington.' Mills shows that the Trinity laboratory was never a physiological laboratory, and that Hales did no work on plant physiology there, and that no evidence exists that he worked there on blood-pressure and animal physiology. The article, although brief, gives a masterly presentation of the evidence, and completely destroys what Mills terms 'a Cambridge myth'.

Mills is survived by his wife, and by their son and three daughters. Their son is now a Consultant Urologist at Boscombe General Hospital. Their eldest daughter married Mr H. Barnes, a University Lecturer in Law at Cambridge and a former Fellow of Jesus College, who died a few days after his father-in-law. Their second daughter is the wife of Professor R. J. Pumphrey, F.R.S., and their third daughter is the Domestic Bursar of Newnham College, Cambridge.

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F. G. Mann

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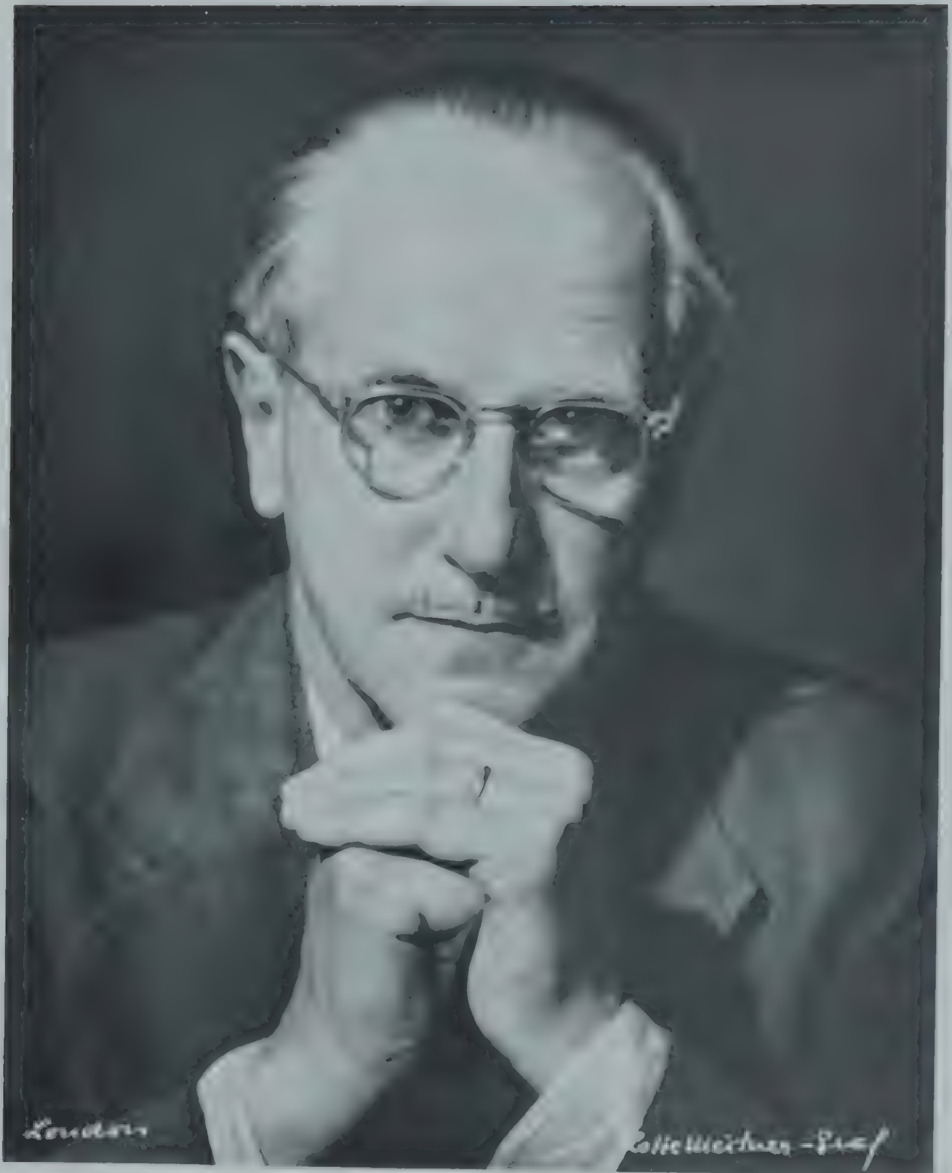
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FRIEDRICH ADOLF PANETH

1887-1958

Friedrich Adolf Paneth was born in Vienna on 31 August 1887. His father, Dr Joseph Paneth, was a well-known physiologist and the discoverer of certain histological cells which still bear his name. His mother Sophie (*née* Schwab) was the daughter of an industrialist who was, for a time, a representative in the Austrian parliament. She had been given a classical education, which was rare at that time for a girl, and had a keen interest in the natural sciences and philosophy. Fritz had two brothers, one older and one younger, and, though both parents were of Jewish descent, the three children were brought up in the Protestant faith. He was first taught at home, but later attended Vienna's renowned Schotten Gymnasium. He was conscientious in his school work and was also endowed throughout his life with an unusual delicacy of touch, so that it was a pleasure to watch his manipulations of familiar as well as of scientific objects. At school he was a good gymnast and later developed more than average skill as a skier, as well as becoming a keen climber. Another major interest was photography. He took up colour photography in 1908, when it was still in the experimental stage; his early achievements in this field would have been impossible without his extraordinary inborn patience. The results compare favourably in selectivity of the view and beauty of tone with many more recent efforts, and form a valuable record of one side of a full life.

The atmosphere of Vienna before 1914 was intensely stimulating, especially in the fields of literature, music, and art, and Paneth's later life shows very clearly the lasting influence of this early environment. When he left school he studied first at the University of Vienna and then with A. v. Baeyer at Munich. He was granted the degree of Doctor of Philosophy by the University of Vienna in 1910 for a thesis entitled 'Über die Umlagerung des Chinidins durch Schwefelsäure' and 'Über die Einwirkung von naszierendem Wasserstoff auf Ei-Albumin'. This work was done under the direction of Z. Skraup, but organic chemistry proved not to be Paneth's chosen field, for his first appointment (1912) was in the Institut für Radiumforschung in Vienna, which had been opened in 1910 and had already become established as the leading research institute in the field of radioactivity. Its internal direction was in the hands of Professor Stefan Meyer, whose assistant Paneth became. The summer semester of 1913 was spent in Soddy's laboratory in Glasgow, and during this time he visited Rutherford in Manchester. Of this time his fellow student, Sir Alexander Fleck, writes:

'He then developed his knowledge of English literature. The foundations laid were put to good use when he came to live permanently in this country. His command of English was impressive and his lectures were spiced with humour and quip, as well as being logically very well constructed.' It may be added that, throughout his life, he remained an outstanding stylist in his mother tongue.

In 1913 he married Else Hartmann, daughter of the historian Ludo Hartmann. She qualified as a doctor of medicine in 1919. They had one son and one daughter. Paneth made his 'Habilitation' in 1913 in Vienna where he remained until 1917, when he went to Prague as assistant to Hönigschmid in the Deutschen Technischen Hochschule. This post was held for only a short time, for in 1919 he was appointed 'Extraordinarius' in the University of Hamburg and, in 1922, moved to a similar post in the University of Berlin. He was appointed George Fisher Baker Lecturer at Cornell University in 1926-27 and some of his assistants accompanied him, taking with them special equipment which enabled active research to be continued. His Lectures at Cornell were a great success and were especially appreciated for the excellence of his English. He spoke not only on his own work, but on ancient alchemy and historical chemistry, in which he had a life-long interest. This is exemplified by his great admiration for Robert Boyle, whose papers he read and re-read. His library was rich in works in this field and his classical training stood him in good stead, for he was able to read the Latin text of an ancient alchemical treatise with the greatest of ease and, with his customary energy and enthusiasm, brought to light forgotten facts and established new relations between known ones. It is also characteristic of Paneth that he devoted a great deal of research and labour to the re-establishment of forgotten reputations. His American visit ended with an extended lecture tour, during which he made a special study of teaching methods.

Paneth was called to the Chair of Chemistry at Königsberg in 1929 and he became director of the Chemical Institute, a singular distinction at a time when this post was held in almost all the major chemical institutes of Germany by an organic chemist. All of his co-workers followed him from Berlin. Although the facilities at Königsberg at first left much to be desired, the necessary equipment was collected and there began what proved to be the most productive phase of his research activities. His work there, which ranged over the fields of free radical chemistry, helium analysis, and the study of meteorites, is discussed later. His interest in historical chemistry also continued and lectures in this field were well attended, though possibly interest centred on the speaker rather than his subject. Questions on historical chemistry were even asked in oral examinations until the students persuaded one of the assistants to intervene. It was typical of Paneth that, after this incident, the questions were dropped, though the lectures continued.

This productive and happy period in Königsberg was cut short by political developments in Germany. Paneth was on a lecture tour in England when

Hitler came to power and he never returned. He saw clearly what was happening in Germany and, because it was diametrically opposed to his own way of thinking, he chose exile and all the difficulties associated with it. Fortunately his personality and scientific standing were such that he would have been welcomed in any of the major research schools of the English-speaking world. It was the good fortune of the Imperial College of Science and Technology in London to be able to welcome him as a guest and make provision for his work and, with the aid of three expert assistants who had accompanied him from Germany, a small but highly productive group was soon established at South Kensington. He was appointed Reader in Atomic Chemistry by the University of London in 1938 and, in 1939, became Professor of Chemistry and Director of the Laboratories in the University of Durham (Durham Division). There, in a setting which had much in common with the older German university towns, a full academic life was resumed until, in 1943, he was put in charge of the chemistry division of the Joint British-Canadian Atomic Energy team in Montreal. It could hardly have been possible to find a better man for this important position. His long and almost unique experience of classical radioactivity enabled him to make major contributions to research on plutonium and many other topics which were studied at Montreal. He proved an ideal leader and his personality made a deep impression on all who worked with him. It was as if an element of old-world courtesy and culture had been injected into an atmosphere which might otherwise have become too intense.

After the war Paneth returned to Durham, established the Londonderry Laboratory for Radiochemistry, and resumed his former research interests. His department was notable for its harmony and his home was a place where his students were always welcome. He reached the retiring age at Durham in 1953 and accepted an invitation to become a Director at the Max-Planck-Institut für Chemie in Mainz. Here he once again set up an active research school, bringing new life into his section of the Institute and stimulating not only the younger men working in his own field, but also many in related fields who came into contact with him. The last five years of his life were, indeed, intensely active, for his personal and scientific qualifications led to many calls for his advice on scientific and administrative problems, among which, and typical of his wide interests, was the selection of a fitting image of Athene to preside over the Max-Planck Institute. It is this last phase which shows us clearly his finest qualities, for he showed those around him that he still had a deep affection for the academic traditions and science of the country where he had spent so many of his early years. He never showed any trace of resentment and this has left a deep impression on all who met him after his return. At the same time, he brought back his appreciation of British ways and institutions.

Paneth seemed to live in, and to establish around him an academic climate in which national differences were irrelevant, while peculiar national excellences contributed to a lively re-appreciation and exchange of values.

Of his scientific work Professor Dingle has written the following: 'He knew no boundaries in science. Though primarily a chemist, his work touched astronomy, physics, geology, and, indeed, whatever happened to be related in any way to the subject of his immediate concern. Nor were scientists—of the past or of the present—merely the authors of scientific papers. They were men and women and the history of science was to him—as indeed it necessarily must be for a full understanding—a part of science itself. It was not merely the variety of his interests that was so thrilling, but also the fact that to him they formed a unity. One never had the feeling that he was changing from one subject to another; he was looking at the same subject from another side.' Another colleague, Dr Hans Götte, has written the following: 'He showed a rare independence of mind in his judgment and thinking, which permitted him to examine facts, events, and people, uninfluenced by the opinion of others or generally accepted views, and to form his own opinion. His humorous, indeed, frequently ironical, criticisms were always tinged by his benevolence and indulgence. With one pregnant phrase, or with a single question, he managed to illuminate a situation in such a way that he could leave it to the intelligence of his audience to form their own views, whose essential features would, however, be determined by his formulation of the problem.'

Paneth's scientific achievements were recognized by many learned societies. He was made an Honorary Member of the American Academy of Arts and Sciences in 1933 and was elected a Fellow of the Royal Society in 1947. In 1952 he became a Corresponding Member of the Austrian Academy of Sciences and, in 1954, an Honorary Member of the Verein Österreichischer Chemiker. He delivered the Liversidge and Hugo Müller Lectures before the Chemical Society, was Halley Lecturer in Oxford, and Schönheimer Memorial Lecturer in New York. An early distinction was the award in 1916 of the Lieben Prize by the Vienna Academy of Sciences. In later years he was awarded the Lavoisier Medal, the Stas Medal, and Liebig Medal, and the Auer Medal.

SCIENTIFIC WORK

Paneth's first scientific publication appeared in 1911 and is an account of experiments on the quinine alkaloids, which were done under the direction of Skraup in Vienna. It is impossible to judge from this single excursion into organic chemistry how he would have fared in this field. He left it almost at once and took up a post as assistant in the Institut für Radiumforschung which was one of the leading centres for research in the new science of radioactivity. His first publication from there was made jointly with Stefan Meyer and described measurements of the intensity of α-rays emitted by purified uranium deposited as a thin film of uranium oxide. It formed part of a programme designed to unravel the decay processes in the uranium series, which were then very incompletely understood. In the same year Paneth described a new method for concentrating polonium, which served to separate it partially from lead and radium *D*. It was based on the observation that polonium in aqueous solution diffused through a parchment membrane very much less readily than lead or radium *D*.

Radium *D* occupied a position of special significance, since it was the only decomposition product of emanation with a sufficiently long half-life to make its separation in weighable amounts from pitchblende feasible. It was known to separate with lead when extracted from radioactive minerals and Soddy had postulated that the two substances were inseparable. There were, nevertheless, conflicting reports in the literature of chemical separations which Paneth proceeded to re-examine. Von Hevesy was conducting similar experiments in Manchester and the two sets of negative results were published in Vienna jointly in 1913.

The paper on the attempted separation of radium *D* and lead paved the way for one of the major developments with which Paneth's name will always be associated. He continued his collaboration with Hevesy and they were quick to realize that, since radium *D* was inseparable from ordinary lead, a small admixture of it with lead would enable them to measure lead concentrations far lower than those which could be determined with ordinary chemical reagents. The lower limit for the microchemical detection of lead was then 3×10^{-9} g whereas, using Ra*D* as an indicator, a sensitivity greater by several powers of ten could be achieved. The method was used by them to measure directly the solubility of lead chromate and sulphide in water. They also pointed out that Ra*D* was not the only radioelement which could be used as an indicator for lead and that the principle could be extended. Either Th*C* or Ra*E*, for example, could be used in the case of bismuth. Paneth's association with Hevesy led to two further publications in 1913, the first on the electrochemical separation of radioactive elements and the second on the separation of polonium by electro-deposition, followed by high temperature distillation. Paneth and Hevesy also collaborated in determining the normal potential of polonium and in separating Ra*D* in visible quantities. The latter was shown to be identical with ordinary lead in its electrochemical behaviour.

During this period of intense activity in the Vienna laboratory Paneth had other interests which were quite distinct from those involving collaboration with Hevesy. Chief among these were the formation of colloids in solutions of radioactive substances and the adsorption of radioelements. The first of these topics proved somewhat intractable, as indeed it still is. Dialysis experiments showed clearly that colloidal hydroxides were formed in several cases by elements which hydrolyzed readily. The diffusion coefficients of polonium, radium *E*, and thorium *B* were found to be smaller in alkaline solution than in acid. This led to the study of the adsorption of radioelements on a number of insoluble salts and oxides. The first results were published jointly with Horovitz in 1914. They included studies of the adsorption of Th*B*, Th*C*, and Ra on such compounds as titanium, chromium and manganese oxides, barium sulphate and chromate, and the silver halides.

Adsorption was strong when the radioelement formed an insoluble compound with the electronegative component of the adsorbing material. Small differences in the mode of preparation of the adsorbing material were found sometimes to alter its adsorbing properties appreciably and it was pointed out that such experiments might throw some light on the composition of solid surfaces. These experiments also explained why radioelements were sometimes precipitated at concentrations far below those corresponding with their solubility products. Paneth returned to these experiments in 1922, when he was at Hamburg, and published with Vorwerk a classic paper on the determination of the surface area of powders by the use of radioactive indicators. This was based on the concept of a kinetic exchange between the molecules (or ions) in the surface of an insoluble solid, such as lead sulphate, and the lead sulphate in a saturated solution in contact with it. If the solution contains a radioactive indicator, then the following relationship holds:

$$\frac{\text{Isotope}_{\text{ads.}}}{\text{Isotope}_{\text{solution}}} = \frac{\text{Element in surface}}{\text{Element in solution}}$$

The ratio on the left-hand side may be found by determining the decrease in activity of a solution containing the radioactive indicator when it is placed in contact with solid. Thus, for example, the fall in activity of a saturated solution of lead sulphate containing ThB when it is placed in contact with a known weight of lead sulphate might be measured. This loss in activity of the solution corresponds with the activity taken up by the solid surface. Since the denominator on the right-hand side of the expression is equal to the solubility of the solid in water, which can be measured independently, the number of molecules of the compound in its surface and the specific surface of the solid can be deduced. The method was first applied by Paneth to lead sulphate and chromate, but was later used for other sparingly soluble compounds. Its usefulness has since been increased considerably by the use of artificial radioelements. Having found a method of determining the surface of solids such as lead sulphate, Paneth was able to take his investigation one stage further by measuring the adsorption of dyestuffs on lead sulphate of known surface area. In this way he was able to estimate the proportion of the surface covered by the dyestuff molecules and to show that only about 30 per cent. of the surface was covered by a monomolecular layer of the dye Ponceau 2R. Later experiments with lead sulphide, bismuth sulphide and phosphate, and barium chromate and several dyes gave similar results: in no case was a complete monomolecular layer formed. Some of the measurements of surface area which had been based on the use of radioactive indicators were checked by direct microscopical measurements and the latter technique enabled Paneth to make a few observations on the adsorption of dyes on other solids, such as artificial silk, diamond, and various other forms of carbon. These experiments were not, however, pursued further. The field at the time offered great scope and the fact that it was dropped may be taken as evidence of Paneth's dislike for repetitive research. In the early 1920's he had other exciting new research in hand and it was to this that he turned his attention in preference to staying in what many would regard as a comparatively 'safe' field. It is also noteworthy that his active work in classical radioactivity came to an end in 1918, though he retained a life-long interest in the subject and his unique experience was to prove of immense value in later years when he was called on to help in work on the transuranic elements. He was associated with Fajans during 1914 in an investigation of the relationship of actinium to the uranium decay series and was able to show that it was not formed directly from radium. The actinium decay products were further examined in collaboration with Stefan Meyer and measurements of the ranges of particles emitted from polonium, ionium, and actinium preparations were also made. The alpha activity of purified actinium sources was noted at this time, and this observation was the forerunner of the later discovery of francium by Mlle Perey. In 1917, however, Paneth, while still at Vienna, made his first experiments in an entirely new field, the use of radioactive indicators to show the formation of unstable hydrides. Shortly afterwards he moved to the Technische Hochschule at Prague and there he completed his work on bismuth and polonium hydrides.

The existence of bismuth hydride, which was unknown at the time, was shown using Th*C*, an isotope of bismuth, as a radioactive tracer. A piece of magnesium was exposed to thorium emanation, when a mixture of Th*B* and Th*C* was deposited on it. When the foil was dissolved in acid the hydrogen evolved carried minute amounts of a gas with the decay characteristics of the bismuth isotope, though no Th*B* was detected at the time. It was concluded that a gaseous bismuth hydride had been formed by the action of nascent hydrogen on the bismuth isotope. The radioactive gas was shown to be decomposed by passage through a red-hot tube. In other experiments, which were reported at the same time (1918), polonium was deposited electrolytically onto magnesium. On dissolving the metal in dilute acid a similar indication of the formation of a gaseous polonium compound was obtained. These experiments actually preceded those on bismuth hydride. After the polonium experiments Paneth wished to check that the activity carried in the gas stream was not attributable to spray, carried over mechanically. He therefore performed an equivalent experiment with thorium active deposit, since neither Bi nor Pb, he assumed, formed a hydride. He must have been disappointed to find that activity did come over in this case also, but fortunately he noticed that the activity decayed with the half-life of Th*C*. This led to the further work on bismuth hydride. Subsequently, bismuth hydride was also prepared by dissolving an alloy of magnesium and inactive bismuth in acid. The amount of hydride obtained was sufficient to yield weighable amounts of the element on decomposition and, with this material, a number of observations were made on the reactions and stability of the new gas.

This success led Paneth to attempt the preparation of a hydride of tin.

Here again the compound was unknown, though the position of tin in the periodic table would lead one to predict the existence of an unstable hydride, SnH_4. The first experiments were made with a tin-magnesium alloy with a composition corresponding with the formula Mg_2Sn. When the alloy was dissolved in hydrochloric acid the gas evolved gave a tin mirror when it was passed through a hot tube. Yields were, however, very poor and Paneth developed a better method involving the electrolysis of stannous sulphate solution with lead electrodes. With a battery of cells he was able to prepare several grams of the new hydride and to make an extensive investigation of its physical, chemical, and pharmacological properties. The study of gaseous hydrides was extended to two other elements, germanium and lead, and earlier results on polonium hydride were amplified. Indications of the formation of a germanium hydride had been obtained as early as 1902. Paneth prepared monogermane by the action of nascent hydrogen on germanium in sulphuric acid solution, a method which gave yields of less than 2 per cent. The compound was, however, analyzed and it was left for Dennis, several years later, to prepare the hydride and its homologues in quantity and make a detailed study of their physical and chemical properties.

It was anticipated that lead hydride, if it existed, would be very unstable and a radioactive tracer method was used to detect its formation. Magnesium was coated with a mixture of Th*B* and Th*C*, as in the experiments on bismuth hydride, and dissolved in acid. The hydrogen evolved was passed through a hot tube, which decomposed bismuth hydride (arising from the presence of Th*C*). When the resulting activity of Th*C* had decayed, there was a small residual activity due to Th*B*, indicating that a trace of a volatile lead hydride had been formed and decomposed. This activity had been overlooked in the earlier experiments on bismuth hydride. Attempts to prepare lead hydride by dissolving an inactive lead-magnesium alloy in acid and to detect it by decomposing the compound in a hot tube were unsuccessful and this led to the exploration of several alternative routes. They included an electrolytic method, similar to that used for preparing stannane, and passage of sparks between lead electrodes in an atmosphere of hydrogen. The results were again negative and an ingenious apparatus was developed in which a lead cathode was used alternately in direct current electrolysis and in sparking. The gas evolved gave a lead mirror when passed through a hot tube. Paneth, however, appears not to have been fully convinced by these experiments and, indeed, other experiments on the production of hydrides in the glow discharge also gave rather ambiguous results. This was particularly so when methane, or some similar organic compound, was introduced into the discharge as a catalyst. Analysis of the products was complicated by the presence of organic decomposition products and probably also of organometalloidal compounds. These experiments are, however, expecially important as they seem to have led to an interest in free radicals, a problem which formed the subject of the next main group of Paneth's researches.

The work on free radicals should be viewed against a historical background. The preparation of free methyl was, indeed, an old problem of chemistry. It was attempted by Kolbe in 1849 and from that time onwards the concept of an organic radical was prominent in many theories of organic reactions, though direct proof that radicals could exist was lacking. What Paneth and his co-workers did was to show by direct experiment that free methyl radicals produced in the gas phase could persist for a small but measurable time before recombining to form ethane. The radicals were produced by pumping the vapour of lead tetramethyl mixed with hydrogen at a pressure of about 2 mm through a tube heated over a short part of its length to a temperature which was sufficient to decompose the lead compound. A deposit of lead was formed and the free methyl radicals produced at the same time could be detected at short distances downstream from the heated zone by their reaction with such elements as tin, antimony, and sulphur. Free ethyl was produced similarly from lead tetraethyl and shown to react downstream with zinc, cadmium, antimony, and lead. These observations were followed up in considerable detail. The mean half-life of the radicals was estimated under controlled conditions of pressure and flow-rate by measuring the time required for the radicals to react with and remove standard mirrors of such elements as lead or tellurium, which had been preformed at definite distances from the heated zone. The recombination of the radicals was also examined and the controlling factors were determined. The possibility of using free methyl and ethyl in chemical synthesis was likewise explored. The products of the reaction of methyl and ethyl with such elements as lead, zinc, mercury, and antimony were shown to be the familiar organometallic derivatives, but antimony gave with either hot or cold mirrors a mixture of the trialkylstibine with what proved to be a tetraalkyldistibine. The new compounds tetramethyl- and tetraethyldistibine were prepared in this way.

Though most of the work in the free radical field was done with methyl and ethyl, Paneth and his co-workers also attempted to obtain evidence for the existence of free radicals of greater complexity. Tetrapropyl lead gave no evidence of propyl radicals, which seemed to be unstable and to decompose to methyl and ethylene. Butyl, phenyl, and the free methylene radical likewise were found to be too unstable to be detected under the conditions used. Indeed, only in the case of the benzyl radical were these exploratory experiments successful. The radical was obtained by decomposing tetrabenzyl tin in a hot tube under the conditions used in studying free methyl, and allowing the decomposition products to react with mirrors of selenium, tellurium, and mercury, benzyl derivatives of which were formed and subsequently identified. These studies were concluded in 1935 by which time they had already had a considerable influence on current work in physical chemistry, especially in connexion with the mechanism of thermal and photochemical reactions in the gaseous phase. Indeed, Paneth's contributions marked a turning point in these fields. His actual experiments are also remarkable for the novelty of the approach and for the elegance of their execution, and they will always be thought of as models of imaginative research.

Paneth's unusually wide interests led him into yet another field which also proved most fruitful. This was the study of methods for the isolation and measurement of small quantities of helium and other gases of the atmosphere. The first experiments of this type were published jointly with Peters in 1926, and were designed to test whether it was possible to transform hydrogen into helium by physical or chemical processes. Several investigators had claimed that this could be done, but, in order to examine the matter, it was necessary first of all to develop techniques for isolating the helium and measuring it in very small amounts. The spectroscopic method was estimated to have a lower limit of sensitivity of 10^{-12} to 10^{-13} g and was the one which was adopted. The chief experiments on the supposed transformation of hydrogen into helium were made with a palladium catalyst and the results were positive. It is evident from the published account of this work that Paneth viewed the result with suspicion, in spite of the fact that the abundance of the helium found relative to the very small amount of neon which was also observed was very much greater than corresponded with an atmospheric origin of the two gases. He examined the point further and was able to show that some helium was derived from the asbestos used in the hydrogen purification train. Even more important was the observation that small amounts of helium passed through both hard glass and hot palladium. These sources of error accounted for what now seems to be a highly improbable experimental result and in all subsequent work with helium these sources or error were rigorously excluded.

Paneth was quick to employ the special methods for helium analysis in other fields, the first of which was the determination of the age of meteorites. In 1926, when this new work started, he had just returned to Berlin after a visit to Vienna, where he had seen the large collection of meteorites in the possession of Professor Czerniak. He was presented with a piece, weighing about 30 grams, taken from the iron meteorite 'Mount Joy'. This formed the start of a collection which he gradually built up until, at the time of his death, it contained more than 100 specimens. In the mid-1920's progress in understanding the origin of meteorites was virtually at a standstill. Many chemical analyses were on record and a broad classification based on composition had been made. The principle of determining the age of rocks from the amount of uranium, thorium, and helium that they contained was already well established, and it was clear to Paneth that his methods for determining extremely small amounts of helium might enable him to find the age of meteorites.

Thus, in 1928, he published his first results on the helium content and age of meteorites and this theme was one which continued to interest him throughout his life. His analytical method was capable of detecting and measuring as little as 10^{-10} ml. of helium, compared with 10^{-7} ml. in Strutt's earlier experiments in the same field. The procedures for the separation and measurement of these small quantities of helium were also steadily developed and refined as Paneth's interest in their applications broadened. Measurements were made on the helium content of large numbers of meteorites of different types and on their uranium and thorium content. It was the determination of uranium and thorium which, in fact, proved later to be uncertain; only at a late stage of the work were reliable methods available for estimating the very small amounts of these two elements which were present. As a consequence, many of the early age determinations had to be revised. The helium estimations have, however, never been questioned and they remain as a model of precise work with very small quantities of gas.

The discovery of the production of helium by cosmic rays led Paneth, in 1952, to re-examine some of his earlier conclusions. He showed that a substantial part of the helium found in iron meteorites consisted of He3 and concluded that this, with a corresponding amount of He4, must have been produced by the action of cosmic rays. His general conclusion, published in 1954, was that the age of most iron meteorites was between 100 and 200 million years, but that some were very much less old. Paneth's interest in this field was exceedingly broad. He collected data on the frequency of meteorite falls and was especially interested in current falls, such as those of the Beddgelert and the Akaba meteorites, both of which occurred in 1949. A specimen of the former meteorite was obtained and subjected to a detailed examination. Paneth's unique collection of meteorites and of literature on the subject is to be kept intact and made available to others working in the field.

In the broader aspects of the subject the very nature of the problem makes any final conclusion a matter of the greatest difficulty, if indeed a final answer can even be found, but the fact that Paneth's interest extended far beyond the actual quantitative determination of helium is typical of his approach to science in general. He was interested in the origin of the universe and was happy to make such contributions as he could to the solution of this great problem.

The methods of separating and measuring very small quantities of helium and other rare gases found other applications in the hands of Paneth and his co-workers. He detected and measured the helium formed in the neutron irradiation of methyl borate. This was the first recorded measurement of a macroscopic product in a nuclear reaction involving neutrons. He also made major contributions to the study of the atmosphere and to stratosphere research. The helium content of atmospheric air on the earth's surface and at heights up to 25 km was determined and no variations exceeding the analytical error of ±0·2 per cent. were found in samples from all over the surface of the globe apart from a helium surplus of about 3 per cent. above the normal at heights between 20 and 25 km. It was concluded that gravitational separation has no appreciable effect on the composition of the atmosphere within these height limits. This work was later extended to 100 km, using steel flasks for the collection of samples. Up to 60 km no change in the composition of the atmosphere compared with ground level could be detected. Above this height, however, analysis of samples showed an increase in the helium and neon and a decrease in the percentage of argon. While it seemed likely that these figures indicated a gravitational separation in an undisturbed layer of the upper atmosphere Paneth was careful to point out that sampling with a fast-moving rocket might cause a separation, and that there was other evidence of frequent and high winds at great heights which would counteract the separating influence of gravity. The studies of atmospheric gases were further extended by the development, in collaboration with J. L. Edgar, of an iodometric method for determining ozone at a concentration of only a few parts per hundred million parts of other gases. It depended on the condensation of ozone on the surface of specially prepared silica gel and separation from other oxidizing gases by fractional distillation. Apparatus was also developed for the continuous recording of atmospheric ozone over periods of 24 hours.

Finally, reference must be made to Paneth's historical and philosophical studies, for these formed a very real part of his scientific life. It was in his student days that he discovered among the family treasures an early copy of a lost treatise by Albertus Magnus, and throughout his life his interest was divided between ancient and modern alchemy. This indeed was evident to any visitor to his library. This interest in the past is well illustrated by an incident which occurred after his appointment to the Chair of Chemistry at Durham. His attention had been captured by the figure of Thomas Wright, who first discerned the true character of the Milky Way and whose *Original theory of the Universe* (1750) was the source that inspired Immanuel Kant to write his *Allgemeine Naturgeschichte und Theorie des Himmels*. Paneth was already familiar with the name of Thomas Wright but was astonished to find that hardly anyone in Durham was. He was able to identify Wright's observatory, a round stone tower which had been built at Westerton, near Durham, and which was known locally as 'the Westerton Folly'. He aroused public interest, with the result that the 200th anniversary of the publication of the *Original theory* was celebrated by public meetings and a memorial plaque was affixed to the tower. This incident is indeed characteristic of Paneth. He had a mind which sought wide horizons. He could be interested in the small things around him as well as in the big. He was fascinated by amber and collected many rare specimens and yet could interest himself in the origin of the universe. Paneth's scientific publications will remain as his lasting memorial, but it is important that we should not forget his personality, which left so vivid an impression on all who knew him.

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John D. Rockefeller. Jr.

JOHN DAVISON ROCKEFELLER, JR

1874-1960

John Davison Rockefeller, Jr, in his autobiographical sketch in *Who's Who in America*, with characteristic modesty omitted to state his occupation. Yet as a young man he had chosen a unique career which he pursued with exceptional devotion and effectiveness throughout a long life. The only son of a man of immense wealth, prospective heir to a fortune so large as to be almost unmanageable, the lifework he chose was to carry on and broaden his father's already extensive philanthropic enterprises and to guide the further disposition of vast sums for the general benefit of mankind. In this career he made his mark in many fields of human activity in the United States and abroad—in art, archaeology, and architecture, the conservation and utilization of natural resources, education, labour relations, natural history, and public health. To the advancement of scientific research he made so great a contribution, not only of money, but also of encouragement and wise direction, that the Royal Society in 1939 elected him to its fellowship as one who, in the words of its Statute 12, had 'rendered exceptional services to the cause of science'. This singular honour, by which he became a regular F.R.S., rather than a Foreign Member like those professional scientists from outside the British Commonwealth whom the Royal Society admits to its companionship, reflects the international scope of his efforts for the advancement of science through support of its institutions.

Mr Rockefeller was born on 29 January 1874, as the fifth child of John D. Rockefeller, founder and head of the Standard Oil Company, and Laura Spelman Rockefeller. The household into which he was born was not only affluent, but also deeply religious and highly conscious of the responsibilities which the older Rockefeller's success in industry had brought upon him and his family. He (the father) was active as a layman of the Baptist communion, through which he was making generous gifts for education and missions. Mrs Rockefeller, though a spirited and independent woman, was also intensely devout. Thus their son grew up in an evangelical community, where he was surrounded almost exclusively by religious people and causes. The lighter forms of social life, including dancing and the theatre, were tabooed and alcoholic liquor was forbidden. To counterbalance these restrictions, characteristic of the dissenting sects in the United States in the 1870's and 1880's, when the younger Rockefeller was growing up, intellectual self-improvement was a moral obligation. Mrs Rockefeller was well read in standard English and American literature, and the family's means made possible extensive tours in Europe and the American West, aimed at educating the children as well as to satisfy their father's fondness for travel. At home, moreover, the family's environment was rapidly broadening. For the sake of his expanding business Rockefeller, Sr, began, while his son was still an infant, to spend his winters in New York City, and in 1884 he settled there permanently. While the family continued to take their religious duties seriously, they now enjoyed the spiritual leadership of a distinguished liberal, non-pietistic pastor, the Reverend W. H. P. Faunce, who later became president of Brown University. Some knowledge of these typically Protestant and evangelical influences of Rockefeller, Jr's boyhood is necessary if we are to understand the combination of a compelling sense of duty, based upon religious faith, with the ever-increasing tolerance and breadth of view that characterized his later life.

As a schoolboy Rockefeller, Jr, was systematic and thorough, winning high grades in the private preparatory school in New York to which he was sent. At an early age he formed the habit of summarizing his reading in a set of small notebooks. When he showed some musical talent his father employed the concert-master of the New York Philharmonic Orchestra to teach him to play the violin. In 1893 Rockefeller, Jr, entered Brown University as a freshman, giving up the comforts of a millionaire's home for quarters as austere as those of an ancient Oxford or Cambridge dormitory. College life somewhat relaxed the restraints of his upbringing. He learned to dance because he felt that he would thus gain needed social experience and grace. This break with family tradition was reluctantly accepted by his parents, who, however, did not approve his desire to sing in the chorus of a students' operetta, or at least to play in its orchestra, and he had to satisfy his love of music by joining a staid string quartet.

In spite of his serious ways Rockefeller, Jr, was warmly accepted by his fellow students and by the élite of Providence and Newport society. Among the homes to which he was invited was that of Nelson Aldrich, wealthy and influential Senator from Rhode Island, whose daughter Abby soon won his heart by her gaiety and ease, and contributed much to his education for life in the social world. In after years he said that his college days were the last in which it was granted him to enjoy the disinterested friendship of his contemporaries, days unalloyed by the intrusions and pressures to which he was afterward subjected as the son of the nation's richest man. In his third ('junior') year his classmates expressed their confidence in his good sense and administrative ability by electing him class president. In his senior year he was manager of the 'varsity football team, winning compliments from the local newspaper on his success in handling details, and a reputation with the players of watching the team's expenditures down to the last penny. He was an excellent student in the standard mathematical-classical curriculum of the day, cautiously diluted with one-year courses in astronomy and chemistry. At graduation in 1897 he was elected to the Phi Beta Kappa Society, an academic distinction roughly comparable to first-class honours in a British university. During his student years his education by travel also continued. In the summer of 1895 he bicycled through the South of England and the Lake District with a college friend. While in London he went to a theatre for the first time in his life. After graduation he again spent a summer in Europe, this time in the company of a sister and some of his father's friends.

Rockefeller, Sr, and his son had assumed from the start that after college the latter would enter the office at 26 Broadway to serve as general aide and to prepare for the responsibilities he would ultimately take over. The young man accepted this role in life with unwavering personal devotion to his father, but with such great admiration of the latter's executive and practical wisdom that he questioned his own ability to measure up to the tasks that lay before him. At the office he was at first left more or less alone to find out what he could do to help his father. Gradually he took over the family's day-to-day financial affairs and his father's relations with the religious and educational organizations to which he was giving assistance. The young man was also made director of a number of industrial companies of which his father was a majority stockholder, and became a trustee of the University of Chicago, which Rockefeller, Sr, had heavily endowed. His marriage to Abby Aldrich in 1901 began a long and very happy domestic relation to which Mrs Rockefeller brought an enthusiastic love of art and music combined with serious interest in her husband's philanthropic work as well as her own.

Some years before Rockefeller, Jr, entered his father's office, the older man had entrusted his philanthropic affairs largely to his associate Frederick T. Gates, a former minister whose astute and vigorous services as executive secretary of the Baptist Education Society had greatly impressed Rockefeller, Sr, and won him a place on the magnate's personal staff. Gates, so shrewd a business man that it was said he could out-deal even Mr Rockefeller himself, was at the same time a fiery idealist with a compulsive urge to help his employer put his wealth to good use. Rockefeller, Jr, now in daily association with this dynamic man, found him an imaginative tutor and guide. For years to come they worked together, Gates often supplying ideas, Rockefeller, Jr, recommending them to his father and directing the organization and execution of whatever projects the latter saw fit to authorize. In this way the son, when only 27 years of age and four years out of college, became a key figure at the start of one of his father's most significant benefactions. Gates's curiosity about the state of medical science had led him to read William Osler's remarkable text-book *The principles and practice of medicine.* Surprised and shocked by its revelation of professional ignorance about the nature, cause, and treatment of many diseases, Gates proposed that Rockefeller, Sr, should found an institute for medical research. The millionaire in his cautious way thought the proposal over for five years, and finally, in 1901, accepted it and asked his son to find ways and means to put it into execution.

The young man wisely began by consulting two New York physicians with strong scientific interests, L. Emmett Holt and Christian A. Herter. Their first discussions at Rockefeller, Jr's dinner-table were followed by the formation of a notable seven-man Board of Scientific Directors (Holt, Herter, William H. Welch, T. Mitchell Prudden, Herman L. Biggs, Theobald Smith, and Simon Flexner) which under Welch's chairmanship chose Flexner as its first director of the laboratories of the Rockefeller Institute. When the Board was ready to erect the Institute's permanent buildings, Rockefeller, Jr, took the lead in finding the site on York Avenue at 66th Street. He closely followed every detail of construction, and was sometimes seen measuring a wall or doorway with a folding rule which he always carried in his pocket. In 1910 the generous endowment which Rockefeller, Sr, had provided, once he and his son were satisfied with the Institute's progress, made it necessary to set up a Board of Trustees, of which Rockefeller, Jr, was naturally a member under Gates's presidency, succeeding the latter as president in 1929 and serving until 1952. As his biographer Raymond B. Fosdick has pointed out, his close association while still a young man with Welch, Flexner and the other eminent members of the Board of Scientific Directors of the Rockefeller Institute was an important factor in his training for lifelong participation in scientific and educational enterprises.

The ever-broadening scope of the family's philanthropies resulted in the creation, in 1913, of the Rockefeller Foundation. Ready by this time for leadership, Rockefeller, Jr, became its first president. After 1917—when a full-time executive, George E. Vincent, took over the presidency—Rockefeller, Jr, was elected Chairman of the Board; in these two positions successively he took part for 23 years in the Foundation's world-wide programme of supporting scientific investigation, education, international exchange of scientists and scholars, and public health research and administration. Mr Rockefeller's strong sense of duty led him in 1910 to undertake public service of a kind that was quite distasteful to one who had seen little of the darker side of city life, and incidentally required great courage. The public authorities, concerned about the spread of organized prostitution in New York City, persuaded him to be foreman of a special Grand Jury convened to look into the situation. After some months of investigation led with notable energy and executive skill, his jury brought in a large number of indictments which resulted in a considerable reduction of organized vice. Mr Rockefeller, however, was not satisfied with mere repression of the outward signs of this serious social malady. His experience as a founder of the Rockefeller Institute had taught him the necessity of investigating the basic causes of physical diseases in order to combat them. Seeing that similar investigation of the basic nature of sexual behaviour and its aberrations must underlie the attack on social disease, he financed a thorough study of prostitution in Europe by Abraham Flexner, and founded a research organization in New York, the Bureau of Social Hygiene, which he supported for many years. An important, though indirect further result of his interest in this problem was the subsequent organization, under the National Research Council, of a Committee for Research in Problems of Sex. Supported by the Rockefeller Foundation and at first led by an eminent psychologist, the late Robert M. Yerkes, the Committee has for three decades added extensive investigations of sex biology and psychology to the research on social problems at first fostered by Mr Rockefeller himself.

Mr Rockefeller gradually came to feel a strong preference for the philanthropic part of the task he had assumed as his father's heir and chief aide. In 1910, when he was 36 years of age, he resigned from the boards of directors of the Standard Oil Company and the U.S. Steel Company because he did not care to share responsibility for the actions of these great industrial organizations when he could not keep in close touch with their administration. He was, however, not yet through with the problems of big business. In 1914 discontent in the mining industry led to a turbulent strike of 9000 miners employed by 20 companies, including the Colorado Fuel and Iron Company, in which Rockefeller, Sr, was the largest shareholder. When violence flared up, the Colorado militia was called out, in the face of protests that the state government was in league with industry to oppress the miners. Rockefeller, Jr, conscientiously troubled by a situation in which his family's influence was involved, personally undertook to investigate the causes of the trouble at the mines. As chief investigator he secured an expert in labour relations, W. L. Mackenzie King, later Prime Minister of Canada. This association began a life-long friendship between the two men. Convinced by Mr King's reports that the workers had been exploited by the mining companies, Mr Rockefeller vigorously proclaimed the right of the employees to share, by negotiation, in determining their working conditions. During a sensational investigation of the Colorado strike by a Congressional committee, Mr Rockefeller, called as a witness and sharply questioned by hostile committee-men, acquitted himself with skill and dignity. This bold stand by a scion of a leading capitalist family was very influential in the subsequent general improvement of labour-management relations in the United States. In another widely publicized controversy, over a charge of financial chicanery by a high-ranking officer of one of the Rockefeller-controlled oil companies, Rockefeller, Jr, again took a stand in favour of the ethical conduct of big business. By these courageous and conspicuous actions he convinced a public long suspicious of the great industrialists, that some of them at least would not tolerate the sharp trading and the exploitation of labour that too often characterized industry in past decades, and in particular did much to counteract unfavourable attitudes toward the Standard Oil Company stirred up by writers of the 'muck-raker' school.

In matters of religion and personal ethics also Mr Rockefeller displayed an increasing breadth of view, and as with civic and scientific affairs learned to investigate problems before taking action. Retaining his own connexion with the Baptists when he built the great Riverside Church overlooking the Hudson River in upper Manhattan, he cordially approved making it interdenominational, with a ministry drawn from various evangelical denominations. Having taken part, shortly after the First World War, in a misguided and over-ambitious 'Interchurch World Movement', when next he supported a far-reaching missionary enterprise it was a serious, well-planned commission to study Asiatic missions to which he gave his more mature consideration.

His attitude toward the 'noble experiment' of national prohibition of alcoholic liquors by constitutional amendment followed the same pattern of broadening outlook and ultimate support of investigation in place of unconsidered action. At first committed to support the prohibition movement by reason of his Protestant evangelical upbringing and church affiliation, he joined his father in giving financial aid to the Anti-Saloon League in several states, and strongly favoured enactment of the Eighteenth Amendment. When, however, its failure to mitigate the evils of intemperance and its bad effect upon the national respect for law became apparent, after conferences in 1932 with a group of prominent citizens he issued over his own signature a vigorous public statement favouring repeal. The next year he organized at his own expense, under the direction of Raymond B. Fosdick, an intensive world-wide study of methods of liquor control that resulted in two influential books favouring an educational rather than coercive attack on the alcohol problem.

One of Mr Rockefeller's boldest enterprises is conspicuously visible to every visitor to New York in the Rockefeller Centre buildings on Fifth and Sixth Avenues from 48th to 51st Streets. This immense project sprang from a relatively modest plan to build a new house for the Metropolitan Opera Company on a part of this site, once occupied by Dr David Hosack's botanical garden and subsequently owned by Columbia University. In an enthusiastic period of national prosperity the project was expanded in 1928 to include a public square adjoining the opera house. To ensure an ample site Mr Rockefeller partly leased and partly purchased considerably more ground than the plan required, expecting to sublease much of it to corporations which would develop the area privately. Delay and finally abandonment of the Opera Company's projected move, followed by the great depression of 1929, left Rockefeller without the support of the enterprise on whose behalf he had entered the scheme, and personally obligated to Columbia University for a lease which would have cost him three million dollars a year. With great courage he decided, in the dark year of 1930, to develop the site himself on a grand scale, with fourteen buildings in all, including two theatres and several skyscrapers, one of which (the R.C.A. Building) would be the largest office building in the city and one of the highest. As in all his undertakings he sought expert help, including in this case eminent architects and a select advisory committee on architectural art and landscaping. The work of construction, carried on during the deepest period of the depression, is estimated to have given employment, directly or indirectly, to 225000 workers. During construction Mr Rockefeller personally passed on general policies and adjusted the inevitable differences between the three participating architectural firms and the builders. He was often on the site, studying blue-prints or with his footrule measuring some detail of the work. In spite of the tensions of the time there were no serious labour disputes and no strikes. Through it all he bore alone a great burden of anxiety, for failure in the immense undertaking would have meant further alarm in business circles, personal loss of prestige for Rockefeller himself, and financial losses sufficient to shake even a Rockefeller's fortune. The completed development in fact ran at a loss for some years but with the return of prosperity finally became profitable. To men of science one of the tall buildings, at 49 West 49th Street, is well known because the Rockefeller Foundation had its headquarters on one of the upper floors.

According to Mr Rockefeller's friend and biographer, Raymond B. Fosdick, of all his undertakings it was the restoration of colonial Williamsburg in which he was most interested and which gave him most personal enjoyment. The idea of recreating the old Virginia capital was born in the lively mind of the Reverend Doctor William A. R. Goodwin, an enthusiastic antiquarian who accepted the rectorship of Bruton Parish Church at Williamsburg with the stipulation that he might restore the edifice, built in 1715, in which Washington, Jefferson, and other distinguished patriots had worshipped. Many other buildings in the sleepy village had survived from colonial times and the seventeenth-century foundations of Virginia's first Statehouse could vaguely be discerned. Memories of these relics of the past haunted Mr Goodwin when he left Williamsburg for another post. Returning as professor in the College of William and Mary, which possessed a building, the oldest academic edifice in the United States, attributed to Sir Christopher Wren, he resolved to continue the work of restoration. Scraping together the necessary funds, he reconstructed first the old powder magazine and then a fine residence. In 1926 he aroused Mr Rockefeller's interest in a project for the restoration of the whole colonial town as a national shrine and centre for the study of colonial history. With his deep-seated interest in architecture and the details of building construction, Rockefeller made himself familiar with the prospective task and after about a year of careful consideration undertook to carry it out. For another year his participation was kept secret, while a staff of architects and real estate experts was organized under the direction of Mr Goodwin and some of Rockefeller's personal aides. The announcement in June 1928 that Mr and Mrs Rockefeller were the donors of funds for this reconstruction gave confidence to the townspeople whose properties and even their way of life were affected by the programme. For several years the Rockefellers spent a couple of months of each year at Williamsburg, following every step of the restoration with intense interest and fostering the development of a group of architects and historians. Their gifts for this work, which called for the restoration or reconstruction of hundreds of homes, shops, and public buildings on their original sites, accurate in every detail and authentically furnished, amounted in all to about fifty million dollars. The excavations made in the course of reconstruction contributed greatly to local eighteenth-century archaeology, and an institute of colonial history now makes Williamsburg a centre of historical research. The people of the United States have accepted with enthusiasm and gratitude this dramatic reminder of their colonial past and of the ideals of the nation's founders. Many thousands of them with their children visit colonial Williamsburg every year.

A lover of the out-of-doors, Mr Rockefeller took great interest in the National Park Service and sought ways in which to help it conserve the scenery and wild life of typical regions of natural beauty. Having a summer residence on lovely Mt Desert Island in Maine, he contributed large parcels of land and much of his own time to the development of Acadia National Park, which includes Mt Cadillac, the highest point on the Atlantic sea coast. The excellent carriage roads of the Park were largely planned by him and constructed under his personal direction. In the West also he financed several projects, of which the most important was the preservaton as a public possession of the scenic Jackson Hole Valley in Wyoming, at the foot of one of the most majestic mountain ranges in the United States, of which the highest peak, the Grand Teton, reaches an altitude of 13 766 feet. Local opposition to the use of the land for park purposes caused him years of negotiation in the face of political dissension which reached the floor of Congress, but he persisted with his plans, returning every summer to enjoy the extraordinary beauty of the valley and the mountains, and finally had the satisfaction of seeing the Jackson Hole region included in the Grand Teton National Park.

Mr Rockefeller fostered the National Parks in many other ways, by encouraging proper road-building in the Yellowstone, helping to save the giant sequoia trees of the Sierra Nevada, in the Yosemite Valley and at Calaveras, and contributing to the purchase of land for the Great Smoky Mountains National Park in North Carolina and Tennessee. One of his most useful undertakings can be seen from New York City, namely, the Palisades Park on the west bank of the Hudson just above Manhattan, which he saved from ruin through quarrying by purchasing land on top of the Palisades for thirteen miles along the river and by construction of footpaths and a motor road at a cost of about 20 million dollars.

His native interest in art was eagerly encouraged by Mrs Rockefeller, whose interest in modern painting and sculpture was expressed by her generous support of the Museum of Modern Art in New York City. His own special hobby was Chinese porcelain, which he began to collect while still a young man. His collection, said to be of unparalleled magnificence, was reserved for his personal satisfaction; to the public he gave an equally sumptuous treasure of ancient art. An American sculptor, George Gray Barnard, had brought together through many years pieces of medieval sculpture acquired largely from ruined and neglected European abbeys and churches. These he set up in a small private outdoor museum in New York, placing the statuary in arcades constructed from parts of two French cloisters. When he had to sell this collection, which had become one of the most significant assemblages of Romanesque and Gothic art in America, Mr Rockefeller bought it, gave it to the Metropolitan Museum of Art, and then persuaded the city to let him build a home for it on high land, which he had himself presented to the municipality, in Fort Tryon Park, above the Hudson at the north end of Manhattan Island. There the architect Charles Collens and the art historian James J. Rorimer erected an impressive building known as The Cloisters, incorporating parts of several French and Spanish churches and monasteries and housing a superb and much augmented collection of medieval art. In this beautifully situated, serene and strangely harmonious structure with its cloister gardens, millions of Americans for whom a visit to Europe can be only a dream have come to feel the inspiration of Romanesque and Gothic religious art as expressed in sculpture, painting and tapestries, carved ivory and jewels. In 1952 by a further handsome gift Mr Rockefeller provided for the upkeep and continuing enrichment of The Cloisters.

If in a sense the treasures of this home of ancient art represent a new country's spoils from the old, Mr Rockefeller more than compensated for them by assuring the preservation of several of Europe's greatest architectural monuments. Visiting the Palace of Versailles, in the summer of 1923, for an evening fête, he and Mrs Rockefeller were distressed by evidences of dilapidation and decay which the French Government, impoverished by the First World War, had not been able to repair. At Fontainebleau also he found severe damage from leaking roofs, and at Rheims, most heart-rending of all, the Cathedral shattered by bombs and gutted by the subsequent conflagration. Before leaving France Mr Rockefeller arranged to have a representative make a quiet survey of the extent of damage to the three buildings, and a few months later diplomatically proffered through the Premier funds sufficient for a thorough restoration. When the work, which cost about 2 850 000 dollars, was completed in 1936, the President of France conferred upon him the Grand Cross of the Legion of Honour.

To the progress of archaeology Mr Rockefeller contributed in many ways. Like his father he supported the excavations of the celebrated Egyptologist, James H. Breasted, for whom also he helped to found the Archeological Institute of the University of Chicago, by a personal gift of one million dollars. His offer to provide a new building for the Cairo Museum fell through because the Egyptian Government would not accept control of the Museum by an international board as Mr Rockefeller stipulated. He did, however, donate and endow the Palestine Archaeological Museum, completed in 1936 at Jerusalem, and at the suggestion of Abraham Flexner he contributed largely to the excavation of the Agora and the reconstruction of the Stoa at Athens.

In 1923 Wickliffe Rose, one of the wisest of Mr Rockefeller's remarkable group of advisers, proposed the creation of an International Education Board. This undertaking, which readily won Rockefeller's interest, in less than a decade aided 57 universities throughout the world, outside the United States, by provision of buildings, equipment, and endowments. Among the notable institutions thus helped were Niels Bohr's institute of physics at

Copenhagen, the departments of mathematics at Göttingen and Paris, the American Academy at Rome, and the American School of Classical Studies at Athens. A full catalogue of the other international benefactions of this far-seeing philanthropist would exceed the space of a brief memoir. Among them was the building of four 'International Houses' as centres for foreign students, at Columbia University in New York and the Universities of California (at Berkeley), Chicago, and Paris. In Japan he financed the rebuilding of Tokyo University's library after its destruction by the earthquake of 1923. He gave the League of Nations its library building at Geneva, and when the United Nations decided to make their headquarters at New York, he presented at a cost of eight and a half million dollars the site on the East River where its buildings now stand. Sharing the concern of sociologists and demographers about the unrestrained growth of the world's population, he made small gifts to various organizations interested in population control. This work his sons now carry on in a larger and more systematic way, by supporting the Population Council through the Rockefeller Brothers' Foundation.

In his own country Mr Rockefeller aided many and diverse good causes by thoughtful benefactions. He made gifts for Negro education in several parts of the South; contributed to the support of the New York Public Library, one of the world's most advanced centres of library administration and research; and through Rockefeller-endowed organizations aided and encouraged the great venture of the 200-inch telescope on Mount Palomar in California. Shortly after the discovery of insulin, at the suggestion of a gentleman whose daughter had been helped by the earliest experimental use of the drug, Mr Rockefeller gave one hundred and fifty thousand dollars, through the Rockefeller Institute, to leading hospitals to provide insulin treatment for free-ward patients and to teach physicians how to use it. He gave land and also endowment funds to the Memorial Hospital of New York, a leading institution for the study and treatment of malignant diseases.

John D. Rockefeller, Sr, lived to see much of his son's success in applying their wealth to the benefit of mankind. Although he began as early as 1900 to relinquish business affairs and to entrust his philanthropic programme to his son, he lived to the age of 97, and in 1937, when he died, Rockefeller, Jr, was 63 years of age. The peculiar relation between these two men; one of whom had amassed a great fortune, and the other was spending it, was made possible by their great mutual affection and trust. The older man watched with tolerance the younger's gradual emergence from the austere puritanism of his youthful environment, and although his son's adult interests must at times have seemed far from his own, he gave his heir a free hand and felt intense pride in his accomplishments.

Because of Rockefeller, Jr's natural modesty and reticence, and also because he always had to protect himself from demands and importunities his private life was little known to the public. One daughter and five sons were born to his marriage with Abby Aldrich, all of whom survive him. Each of the sons now has an active part in one or another of their father's major philanthropic enterprises. One of them, Nelson Aldrich Rockefeller, has given distinguished service to the national government, especially in regard to South American relations, and as Governor of New York State is much in the public eye. After 47 years of happy marriage Abby Aldrich Rockefeller died in 1948. Three years later Mr Rockefeller married as his second wife Mrs Martha Baird Allen, widow of an old friend and college classmate. After about 1950 he gradually retired from active work. He died at Tucson, Arizona, on 11 May 1960.

John D. Rockefeller, Jr, would have been the first to say that in his extraordinarily useful career as a philanthropist he benefited by the counsel and executive services of several very able advisers; but his ability to hold such men in the long-term conduct of his life-work, and to enlist many other competent workers in each of his large enterprises, is a measure of his own character and talents. Those who were closest to him have all emphasized his moral courage, his quiet diplomacy, his painstaking care of details and his willingness to use the enormous power of his wealth to stimulate action for good ends without imposing autocratic control. His best epitaph perhaps is found in the words of his earliest associate in philanthropy, Frederick T. Gates: 'I have known no man who entered life more absolutely dominated by his sense of duty, more diligent in quest of the right path, and more eager to follow it at any sacrifice.'

The author of this memoir is deeply indebted to the superb biography by Raymond B. Fosdick, *John D. Rockefeller Jr., a Portrait*, New York, Harper and Brothers, 1956.

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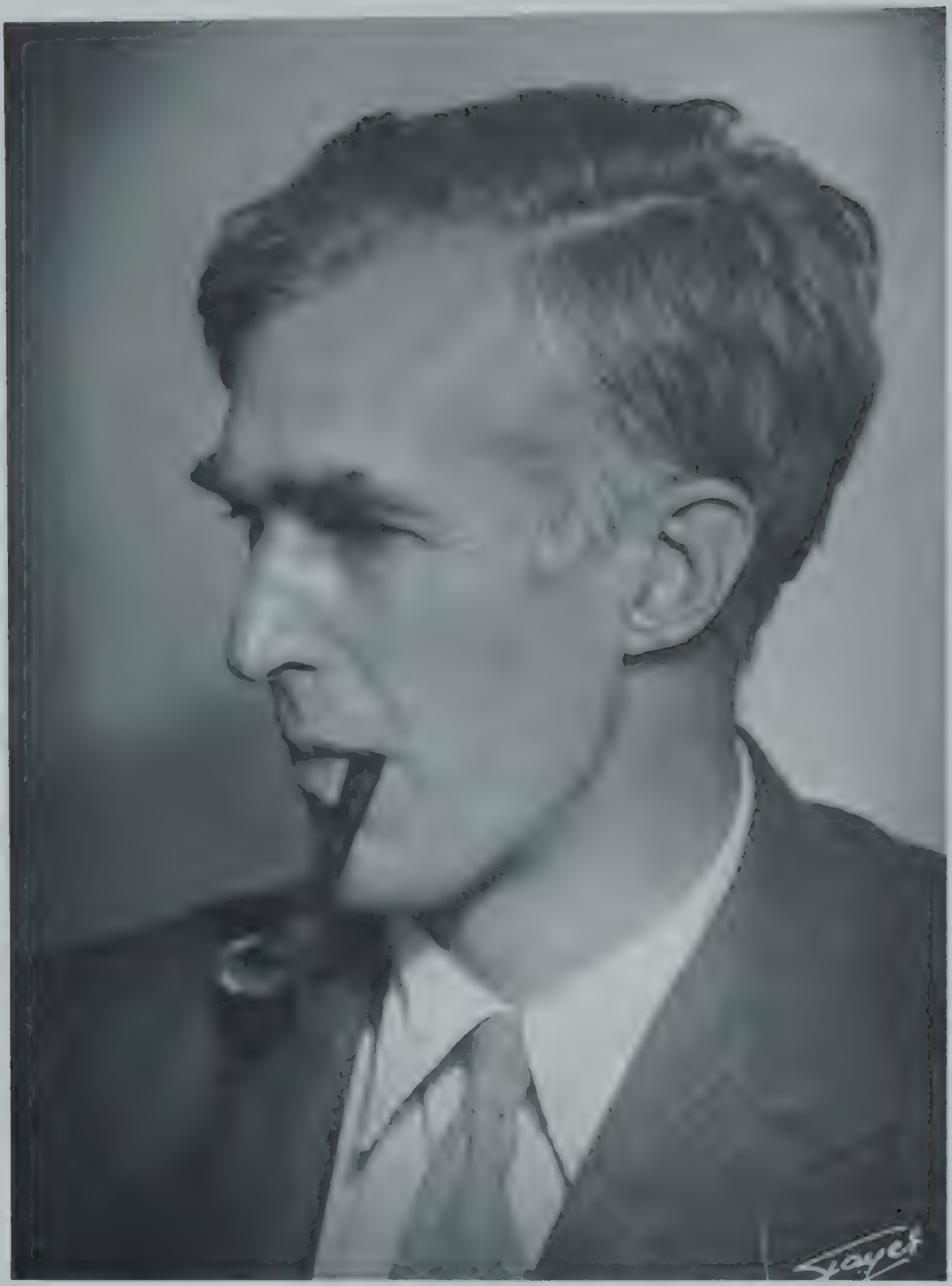

HWBSkinner

HERBERT WAKEFIELD BANKS SKINNER

1900-1960

Herbert Wakefield Banks Skinner was born on 7 October 1900, at Ealing on the western outskirts of London and died at Geneva while on a visit to the European Organization for Nuclear Research on 20 January 1960. He was the only son of George Herbert and Mabel Elizabeth Skinner. His father was a member of the directorate of the shoe firm, Lilley and Skinner, and was in many ways a remarkable man. The interests of George Skinner lay more with motoring and engineering than with commerce and he appears to have been one of the first in England to own a motor-car. He brought over from France a Léon Bollée car; a type which originated as long ago as 1896. His interests in the mechanism of the internal combustion engine led to the invention of a new type of carburettor which was the subject of a British patent application in February 1905 and which was built in the first instance in his own house. Later the demand for an efficient carburettor led to collaboration with his brother, T. C. Skinner, and together they formed a company in August 1910. This company, known as Skinner Union, manufactured the S.U. carburettor, and it is of interest that some of its original features are retained in the present-day model.

George Skinner had his own ideas on children's education and did not believe in early schooling. His son Herbert was accordingly 9 years old before he entered Durston House School at Ealing. The son had evidently inherited some of his father's ability for, five years later, he won a mathematical scholarship to Rugby School and entered School House in September 1914. His progress at Rugby was marked by success: he won prizes both in mathematics and natural sciences and, when he left school in 1919, he was head boy of the science side and also head of School House. In October of that year Skinner entered Trinity College, Cambridge. He had won both a major and a minor leaving Exhibition from Rugby in mathematics and the natural sciences respectively. After a successful undergraduate career he obtained in the summer of 1922 a first-class in Part II of the Natural Sciences Tripos, having earlier been placed in the first class of Part I of the Mathematical Tripos. For the next five years Skinner carried out research work in the Cavendish Laboratory and for part of this time he held the Coutts Trotter Studentship of Trinity College.

It was in 1919 that Rutherford had returned to Cambridge as the Cavendish Professor of Physics and with Chadwick began a new attack on the problem of the disintegrating nucleus. It was natural, therefore, that

Skinner's early researches should be concerned with some aspect of radioactivity. As a research student with C. D. Ellis (now Sir Charles) he worked on the β-ray spectrum of radium B and C. Three papers were published on this work in 1924. Soon afterwards he abandoned nuclear physics, not to return to it again until the exigencies of the second world war called for the study of nuclear fission and isotope separation, and joined with P. Kapitza in the investigation of the Zeeman effect in strong magnetic fields. It was at this time that Kapitza had just succeeded in producing those exceptionally intense magnetic fields for which he is well known, and the work with Skinner on the Zeeman effect in the spectrum of ionized calcium was one of the first uses to which the new technique was put. Later Kapitza employed the strong magnetic fields in his extensive series of investigations on magnetoresistance effects in metals.

After the work with Kapitza Skinner was awarded an 1851 Exhibition Senior Studentship and undertook, on his own account, an investigation of the polarization of the monochromatic radiations emitted from mercury vapour under bombardment by a well-defined electron beam. It was found that in the absence of a magnetic field a considerable degree of polarization was produced, in most cases with the electric vector of the radiation in the direction of the electron beam. The depolarization effect of a transverse magnetic field was also studied. Later, with E. T. S. Appleyard, the influence of the energy of the electron beam in producing the polarization was determined and it was shown that there was a maximum of polarization when the energy of the electrons was a few volts above the excitation potential. Skinner was never satisfied merely with the measurement of physical effects; he always sought an explanation of his results in terms of fundamental theory. It is characteristic of his papers that the sections headed 'discussion' are long and carefully written. From the time of these experiments onwards a connexion is visible throughout the sequence of Skinner's published researches until they culminated in the work on soft X-ray emission and absorption in metals which undoubtedly constitutes his main contribution to physics. In all this work the electron beam is used as one of the main tools of his experimental researches.

After leaving Cambridge in 1927 Skinner joined the Physics Department of the University of Bristol where he held a newly established Henry Herbert Wills Research Fellowship. The Physics Department of Bristol was transferred at that time to the new buildings known as the Royal Fort and the H. H. Wills Physical Laboratory. It must then have been one of the largest and best equipped physics buildings in the country. Professor A. M. Tyndall who was head of the Department at the time had collected around him some of the ablest experimental physicists in England as well as a number of overseas theoretical physicists who have since become well-known figures in their subject. Skinner threw himself wholeheartedly into the activities of the laboratory. His encouragement of free discussion, his wide reading and his critical—even if occasionally hypercritical—analysis played an important part in the growth of the reputation of the laboratory at this period. In this environment and with a strong urge to do work of originality Skinner undertook an experiment intended to illustrate the wave nature of the electron, a concept which at that time was a novelty. This investigation was not in line with his main research development and, due partly to the inherent difficulty of the experiment and partly to the inadequacy of the techniques then available, little came of this work. Having abandoned this research, Skinner undertook an investigation of the excitation potentials of the light metals, lithium and beryllium, but five years elapsed between his joining the laboratory and the publication of his first substantial paper from Bristol.

It was during this period in 1931 that Skinner married Erna Wurmbrand and lived for a time in one of the houses of the terrace known as the Paragon. Later he built and lived in an attractive house of modern design overlooking the Avon Gorge. Although reserved in many ways, Skinner was an extremely hospitable man and many of those working in the physics laboratory at that time will remember pleasant evenings spent in this house.

The experiments concerned with the excitation potentials of lithium and beryllium prepared the way for his outstanding work on the soft X-ray emission spectra of light metals. In these early experiments the intensity of the radiation emitted from the metal surface under electron bombardment was measured for different values of the accelerating voltage of the electrons. Changes of slope in the curves representing the intensity of emission against the voltage indicated the values of the excitation potentials. It was inferred that these critical voltages determined the least energy required to remove an electron from the 1*s* state of the atom when this atom formed part of the solid metal. The interest of these experiments lay in the difference between energies so measured and the corresponding energies obtained when the atom was in the free state as in the vapour phase. Some unexpected results were obtained and much ingenuity was shown in providing an interpretation for them. It has to be recorded that the rather elaborate theory which was proposed had to be modified in the light of later results obtained by Skinner himself.

The experiments on excitation potentials and soft X-rays occupied the period from 1932 to the time when Skinner left Bristol in 1940 to undertake scientific work connected with the war effort. He did not return to Bristol and never again did experimental work in this field. He became involved in the development of radar and, in the later years of the war, with one aspect of the atomic energy programme. In 1954, stimulated by the appearance of new measurements on the emission of soft X-rays from transition metals by workers at the Massachusetts Institute of Technology, Skinner published results of work carried out just before the outbreak of war dealing with the same elements. This was the last scientific paper published under his name. The significance of his soft X-ray work for the theory of metals will be considered later in this memoir but for the moment we follow the progress of his scientific career through the war years to the time of his appointment as Lyon-Jones Professor of Physics at the University of Liverpool in 1949.

During the summer of 1939 plans were prepared by Dr R. A. Watson-Watt (now Sir Robert) on behalf of the Air Ministry for eight teams of physicists from British universities to obtain training in radio-direction finding (radar) by working on the chain stations on the south and east coasts of England. Skinner took charge of the group of physicists from Bristol University and went to the station at Poling in Sussex. Shortly afterwards he was entrusted with an important mission in the Shetlands. This arose in the following way: Sir James Somerville, in charge of Admiralty Radar, asked for assistance in providing radar cover against submarines approaching Scapa Flow. Professor J. D. Cockcroft (now Sir John) took his group of physicists to Bawdsey to carry out experiments with the first experimental set built for the detection of low-flying aircraft. They were able to show that this equipment could detect surface submarines at a range of several miles, and a modification of this set called C.D.U. (Coast Defence U-boat) was adopted for this purpose. Sir James Somerville asked for sets to be installed on Sumburgh Head, Fair Isle and Orkneys by Christmas. The improvisation of the radar set was carried out at Christchurch and Skinner took charge of the party to build a station at Sumburgh Head, a very exposed cliff on the southern tip of the Shetlands. The difficulties which had to be faced in the bad weather and gales of the late autumn of 1939 were enormous. In spite of indifferent health Skinner completed the erection of the station by the first week of December 1939.

From the Shetlands Skinner went to the Telecommunication Research Establishment then located at Swanage and, with other physicists, started work on the development of microwave radar. They worked in stables in the grounds of a house on the hill above Swanage. Skinner's most important contribution was the microwave crystal mixer which mixed the incoming signal with the signal from a local oscillator. He was also largely responsible for the development of a device which made it possible to send and receive signals on a single aerial. He was one of the first to work on the detection of submarines by microwave radar and, by 7 November 1940, had succeeded in picking up a submarine at a range of 6 miles in Swanage Bay by means of a crude set working in the grounds of Leeson House. When the Telecommunication Research Establishment moved to Malvern Skinner went with it and continued to work with the microwave group during the period in which it developed anti-submarine microwave radar, air interception microwave radar, and the system which was used for the guidance of bombers.

Skinner's association with the nuclear energy programme began in 1943 when he left Malvern to join a group of British physicists led by Oliphant, to whom he acted as deputy, working at Berkeley, California, under the late E. O. Lawrence on the electromagnetic separation of the uranium isotopes. The principle underlying this method of isotope separation is essentially that of the Aston mass spectrograph. The 180-inch magnet which had been built for the Berkeley cyclotron was developed into the first large-scale electromagnetic separator and this apparatus served as a pilot plant for the later enormous construction at Oakridge.

The British Atomic Energy project was founded in January 1946 and the Harwell airfield was chosen as the site of its Research Establishment. J. D. Cockcroft was appointed its first director but remained at Chalk River, Canada, until June 1946. Skinner, as the senior scientist, directed on behalf of Cockcroft the early stages of the building of the establishment. With a small number of colleagues he set about recruiting staff and building a research laboratory. During the early months the construction of a large cyclotron, a 6 MeV Van de Graaff generator, and the 100 kilowatt Gleep reactor were started. In addition to scientific administration, Skinner had some responsibility for providing housing quarters for the growing staff. Some 200 prefabricated houses were hastily erected and arrangements had to be made for schools, canteens and other requirements for the Research Establishment. A good deal of improvisation was called for and Skinner carried this through forcibly and successfully although not always in a way to the liking of the official administrators. Skinner's particular creation at Harwell was the General Physics Division which covered a wide range of scientific activities such as mass spectrometry, X-ray and neutron diffraction, cloud chamber and emulsion techniques for high energy particles.

In addition to his general responsibilities as Cockcroft's deputy, Skinner had also at this stage particular responsibility for research on the separation of light elements by gaseous diffusion, for the preparation of radioactive and stable isotopes as tracers, for electronics research and for the construction of the large electromagnetic separator. This large separator was based broadly on the design of the separator at Berkeley of which Skinner had direct knowledge as a result of his earlier work on that project.

The task of building the large cyclotron expeditiously was not easy: at that time the principle of frequency modulation had not been tested in practice and so there were many discussions about the size of the magnet before finally deciding to use one of 110 in. diameter. The practical problems of obtaining from industry quickly and economically the 670 tons of steel for the magnet were tackled energetically. At that time the conversion of buildings to laboratories proceeded steadily but exasperatingly slowly to those wishing to use them. In contrast the contractors demonstrated impressively the speed at which large holes in the chalk could be excavated by the progress they made with the foundations of one of the Harwell reactors. Skinner was quick to take advantage of this by having a large pit excavated for the cyclotron; thus overcoming problems of shielding as well as expediting the work.

Before he left Harwell to take up the Lyon-Jones Chair of Physics at Liverpool, Skinner had become chief physicist and his division had reached a high point of efficiency. The group leaders working under him were given very considerable freedom to plan their own researches and to make their own decisions and he was always ready to give an able young man his head. He made it his business, however, always to know what was going on and to push his own point of view in any important discussion.

In 1949 Skinner joined the University of Liverpool to occupy the Chair of Physics vacated by the retirement of Sir James Chadwick. In his first year as Professor, apart from departmental duties, he was mainly occupied with the completion of the large cyclotron which had been begun by his predecessor. This apparatus, when it finally came into operation, gave a beam intensity larger by a factor of at least 1000 than that achieved hitherto by any cyclotron of comparable size. Skinner did not suggest or even actively contribute to this particular design but it was due to his initiative and enthusiasm that the earlier ideas of J. L. Tuck were examined theoretically with such success by Le Couteur, and that the practical realization of Le Couteur's theory was carried out at the Nuclear Physics Laboratory at Liverpool.

In later years Skinner was principally concerned with the organization of research and teaching in his department and did not attempt to carry out original research on his own account As might be expected, he played an active part in general University affairs and, for the last few years of his life, was Chairman of the Senate's Departmental Grants Committee. During his tenure of the Chair at Liverpool Skinner also took a leading part in the development of the European Organization for Nuclear Research (C.E.R.N.). The physicists at Liverpool University under his guidance were actively concerned with the design of the C.E.R.N. synchrocyclotron, and Skinner himself was appointed Chairman of the United Kingdom's panel of C.E.R.N. which helped to steer the organization along the right lines in its formative years. Skinner also took an active interest in the affairs of the Royal Society. He had been elected a Fellow in 1942 and was serving as a member of the Council at the time of his death.

Turning now to Skinner's main contribution to physics as revealed in his published papers, it is worthwhile recalling the position of the theory of metals at the time when he began his well-known research into the nature of soft X-ray emission from light metals. Many of the principal features of electronic states in crystals were already understood in 1933. The foundations of the theory had been laid by Pauli, Sommerfeld and Bloch some five years earlier and in 1930 there appeared the paper in which Brillouin described those regions of momentum space which have ever since been known as Brillouin zones. The difference between metals and semi-conductors had been made clear by the theory of A. H. Wilson; in metals the zones are only partially filled by electrons, in insulators they are completely filled and in semiconductors the zone is again completely filled at the absolute zero temperature but at finite temperatures some electrons are able to escape into a conduction band of higher energy. Yet although the wave-mechanical formulation of the theory of metals had been firmly laid by 1933, detailed applications to account for the properties of particular metals had not yet been made to any great extent. Within a year of two of this time, however, there appeared the well-known work of Wigner and Seitz on the constitution of metallic sodium, Mott's theory of the transition metals, and the first attempts to understand the properties of alloys in terms of the new theory. In the course of these developments between 1933 and 1936 the quantity known as the density of electronic states, and generally denoted by the symbol $N(E)$, was recognized to be of special interest. More precisely the quantity $N(E)\mathrm{d}E$ denotes the number of electrons in the valence band with energies lying between E and $E+\mathrm{d}E$. It was the great merit of Skinner's work that he obtained a very direct experimental determination of this quantity for a number of pure metals of low atomic weight.

Although Skinner's name will always be associated with this beautiful verification of the wave-mechanical theory of electrons in metals, for its scope, its accuracy and the excellence of its technique, yet the earlier pioneering work should not be overlooked. As early as 1930 a photograph of the emission band from metallic beryllium was published by Söderman, which, though of poor quality by comparison with later photographs, was nevertheless of approximately the correct width. Inspired by this experiment, W. V. Houston gave a theoretical interpretation of the reasons for the appearance of a band rather than a line when the emission arises from the element in the solid state. Houston's paper was the first to give many of the features of what is now believed to be the correct theory of the soft X-ray emission from metals. In particular, he noticed that the width of the emission band was approximately given by Sommerfeld's formula for the energy of the Fermi limit of a free electron gas; also that, if the conduction electrons are approximately free, the emission band should have a sharp edge and that the intensity of the band is given by the product of $N(E)$ with a transition probability $p(E)$. His calculation of the probability $p(E)$ was, however, inadequate.

Skinner's first work on the photographic determination of soft X-ray emission from metals was carried out in collaboration with H. M. O'Bryan at the Massachusetts Institute of Technology. In their first publication in October 1933 they gave results of measurements of emission bands for the following light elements in the solid state: lithium, beryllium, carbon, sodium, magnesium, aluminium and silicon. Later publications gave further details and a more closely reasoned account of the theoretical implications of their observations.

The choice of these light metals for the first experiments was a happy one for even now it is only with these metals that the emission spectra allow of a fairly clear and unambiguous interpretation. The soft X-ray emissions from the elements of the fourth period of the Periodic Table which have so far been examined, for example, calcium to nickel and also copper and zinc, although showing band emission, do not show the sharp edge which is such a striking feature of the spectra of the light elements. The last paper which

Skinner wrote dealt with the emission bands from the elements of the fourth period and, in this paper, he put forward the view that the observed emission is due to transitions from the *d*-band to the X-ray level. In some cases the X-ray level is in the *L* shell, in others in the *M* shell and both types of spectra were examined with great care. The lack of a sharp edge at the high frequency limit Skinner attributed to radiationless Auger transitions which, since they reduce the lifetime of the excited state, widen the emission line due to each transition in accordance with the uncertainty principle of quantum mechanics.

Although this last paper was written in 1954, the experimental work on which it was based was carried out in 1939 not long after the second major contribution which Skinner made in the field of soft X-ray spectroscopy; namely, the observation of the absorption spectra of various metals in this region of the spectrum. This work was carried out in collaboration with J. E. Johnston and illustrates Skinner's extraordinary powers as an experimentalist. Some extremely difficult technical problems had to be solved to obtain sufficiently accurate absorption spectra. The absorption of radiation in the region of the spectrum 100 to 300 Å with which this work is concerned arises from the transitions of electrons in the *K* level in the cases of lithium and beryllium, and from the *L* or *M* levels for heavier metals to states in the conduction band just beyond the Fermi limit. For those metals for which it is possible to obtain a clearly defined absorption edge, for example, lithium and magnesium, these results, together with the observations of the emission bands, give information on the whole course of the density of electronic states in the valence or conduction bands.

One example of Skinner's insight and tenacity of purpose in his experimental research is worth recalling. The first theory of the emission bands for light metals indicated that, although the low frequency end of the band would not have the sharp edge of the high frequency end, it nevertheless would terminate at a well-defined wavelength which should be recognizable on photometric curves obtained from the exposed plates. Skinner noticed that this low frequency end showed a general tailing off which he was able to prove was not an instrumental effect. He proposed a theory, which has since been worked out in detail by others, that any vacant level in the conduction band was broadened by Auger transitions between other electrons in the same band. The main feature of this theory was that only a few such transitions can take place for states near the top of the Fermi limit while many are possible for states of lower energy in the band. It thus accounts for the sharp upper edge and yet provides an explanation of the tail. The detailed study of this theory by other workers provided one of the first examples of intrinsic screening between free electrons. An adequate explanation of these results had to await the development of the theory of plasma oscillations in metals.

Although Skinner will most likely be known to future generations of physicists mainly through his work on soft X-ray emission and absorption by metals, yet by the present generation his work in other fields, not embodied in published papers, will be widely appreciated. He played a large part in building up the Research Establishment at Harwell, particularly in the early days, and later, while head of the Physics Department at Liverpool, stimulated and organized much of the research work in nuclear physics carried out there. His drive and determination had a widespread effect, while his helpful kindness to many junior members of his staff will be remembered with gratitude. Skinner had not many interests outside his work though he read widely and even wrote a certain amount of verse, some of which was published in the Harwell magazine *Harlequin*. A few lines of a sonnet written in 1941 called 'Wartime Research 1940' may be quoted as showing, not only his powers in this respect, but also something of his outlook:

> And so alone,
> we, fighting every inch of the way,
> against those ingrained elephants of inertia,
> against the prejudice and the hardened pride
> of self-established, self-supporting systems,
> we fought (through forests, thick with self-satisfaction)
> to shorter electromagnetic wavelengths.

The feelings of those who knew Skinner personally were well-expressed in a tribute from a colleague who wrote at the time of his death: 'What we shall remember about Herbert Skinner above all is the way he stood for his convictions. As a good scientist, he would listen to an argument and was willing to be proved wrong, but he would take no notice of whether his views were popular or whether he was in a minority. It was utterly irrelevant whether his views or his actions were fashionable, whether they would result in personal advantages or credit to him, or whether they would get him into trouble. In technical matters, as in questions of policy, only the substance counted and he was not impressed by authority or convention.' Skinner did not invariably succeed in establishing happy relations with all his colleagues but, behind his unconventional and often unpredictable attitudes and opinions he was essentially a gentle and kindly man, and his unexpected death was a grievous loss to science, to University life and to his many friends.

I wish gratefully to acknowledge the assistance I have had in the writing of this memoir. Mrs Skinner supplied me with information of her husband's early life, and Sir John Cockcroft, F.R.S. and Dr Pickavance with notes of Skinner's war work and activities at Harwell. To Professor A. M. Tyndall, F.R.S. and others too numerous to mention I am grateful for letters, reminiscences and helpful comment.

H. JONES

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C. T. R. Wilson

CHARLES THOMSON REES WILSON

1869-1959

CHARLES THOMSON REES WILSON was born on 14 February 1869 at the farmhouse of Crosshouse near Glencorse in the Pentland Hills near Edinburgh. He was the youngest of eight children by the two marriages of his father John Wilson, a very progressive sheep farmer, whose family had farmed in the neighbourhood for generations, and who himself had published papers in the *Journal of the Highland and Agricultural Society* on various experiments in farming. Charles's mother, Annie Clark Harper, was a second cousin of her husband and came of a Glasgow family, who in the eighteenth century had been prosperous thread makers and muslin manufacturers, as well as burgesses of the city, but whose business had latterly declined. Some of the family attended Glasgow University. The Harpers had literary inclinations: George Harper, Professor of English Literature at Princeton and a distinguished author, was a second cousin of Charles.

When Charles was 4, his father died aged 53 and his mother was left with three young children of her own and four stepchildren. The whole family, which was a very united one, moved to Manchester to be near Mrs Wilson's parents, who had moved there from Glasgow. The elder brothers were determined that their two young half-brothers, Charles and George, in whom they had great faith, should achieve the university education which they themselves had missed. One of them, William, went into business which took him in 1877 to Calcutta, where he rose to be a partner in his firm, a town councillor and a member of the Calcutta Chamber of Commerce. For many years he supplied the main part of the family income. He died of tuberculosis in 1892 at the age of 35, on his way home to England.

Charles attended Greenheyes Collegiate School at Manchester from the age of 9 to 15. There was no science taught at the school, but Charles and his brother George spent a great deal of time studying beetles and pond life and made good use of a microscope which Charles had been given when he was 13; they constructed a microtome and became skilled in preparing zoological and botanical specimens.

When Charles was 15, he and his brother went to the island of Arran in the Clyde, and he commented later that his visit to North High Corrie was a wonderful revelation to him of the beauty of the world and inspired him to study nature in all its aspects. If it was this first visit to the Scottish mountains which did much to arouse Charles's desire to investigate natural phenomena, it was another visit ten years later, this time to Ben Nevis, which defined the field of his life's work, that of condensation phenomena and atmospheric electricity.

Through the financial help of his half-brother William in Calcutta, Charles was enabled to enter Owens College, Manchester, in 1884, when he was 15, registering as a medical student but intending to take a science degree first. For three years he studied physics, chemistry, botany, zoology and geology, taking a B.Sc. when he was 18, followed by a fourth year studying philosophy, Latin and Greek. In 1888, at the age of 19, he won an entrance scholarship at Sidney Sussex College, Cambridge, and by the time he arrived there he had definitely decided to be a physicist; so he took physics, together with chemistry, geology and botany in Part I of the Tripos and physics and chemistry in Part II. His half-brother William died three months before Charles took his Cambridge degree in 1892, and Charles wrote of him: 'His encouragement and faith in me and my desire not to disappoint him had been among the strongest influences of my life.'

It now became essential for him to help his family, who were now back in Scotland within a few miles of their old home. He stayed on in Cambridge after taking his degree, doing a certain amount of demonstrating in physics and chemistry, and also set to work on a research problem. This made use of an optical method of studying the distribution of a substance in a liquid which was kept hotter at the top than at the bottom, comparing it with the behaviour of a gas. However, in 1894 he felt that his career in Cambridge was very precarious and the burden of coaching prevented him making progress with his researches, so he accepted an offer of a post as assistant master at Bradford Grammar School. Though he enjoyed teaching boys and later spoke of the pleasure he had in introducing them to geology, he realized that he would never get any research done and that he must escape before it was too late to change his career again. So he returned to Cambridge, to live in very dreary lodgings and with no prospect of definite employment but with the determination that he must make another attempt to get some experiments done. Later he used to comment on the transformation of his outlook when he was given work demonstrating to medical students by Dr Fitzpatrick: this gave him just enough to live on and a connexion with the Cavendish Laboratory, where J. J. Thomson had already, at the age of 38, held the Cavendish Chair for ten years. In this and the following year, C. T. R. Wilson had two experiences which were to shape his research career: these are best told in his own succinct words, the first taken from his Nobel Lecture delivered in Stockholm, 12 December 1927:

'In September 1894 I spent a few weeks in the Observatory which then existed on the summit of Ben Nevis, the highest of the Scottish hills. The wonderful optical phenomena shown when the sun shone on the clouds surrounding the hill-top, and especially the coloured rings surrounding the sun (coronas) or surrounding the shadow cast by the hill-top or observer on mist or cloud (glories), greatly excited my interest and made me wish to imitate them in the laboratory.

'At the beginning of 1895 I made some experiments for this purpose—making clouds by expansion of moist air after the manner of Coulier and Aitken. Almost immediately I came across something which promised to be of more interest than the optical phenomena which I had intended to study. Moist air which had been freed from Aitken's dust particles, so that no cloud was formed even when a considerable degree of supersaturation was produced by expansion, did appear to give a cloud if the expansion and consequent supersaturation exceeded a certain limit. A quantitative expansion apparatus was therefore made in which given samples of moist air could repeatedly be allowed to expand suddenly without danger of contamination, and in which the increase of volume to be made could be adjusted at will.'

The second experience is described by him in an article 'Ben Nevis sixty years ago' in the magazine *Weather* in 1954:

'On the afternoon of 26th June, 1895, I was standing on the summit of the Carn Mor Dearg. The sky had become overcast as I climbed up from the allt a'Mhuiliunn and mist hid the top of Ben Nevis; there was a faint muttering of distant thunder. Suddenly I felt my hair stand up; I did not await any further developments, but started to run down the long scree slope leading to the bottom of the corrie. The storm broke overhead with a bright flash and loud thunder just after I had left the summit. This experience drew my attention very forcibly to the magnitude of the electric field of a thundercloud and to its sudden changes.' C. T. R. Wilson ends this article with the following words: 'The whole of my scientific work undoubtedly developed from the experiments I was led to make by what I saw during my fortnight on Ben Nevis in September 1894. It is hardly necessary for me to say that these experiments might have had little result had it not been that they were made in the Cavendish Laboratory at the beginning of the wonderful years of the discovery of the electron, X-rays and radioactivity.'

'C.T.R.', as he was soon to become affectionately known, held the Clerk Maxwell Studentship from 1896 to 1899, after which he spent a year working on atmospheric electricity problems for the Meteorological Council. In 1900 he was elected a Fellow of his College, Sidney Sussex, and appointed a University Lecturer and Demonstrator. He was elected Jacksonian Professor in 1925 and remained in Cambridge until 1936, two years after his retirement from the Chair. The Royal Society elected Wilson to the Fellowship in 1900, and awarded him the Hughes Medal in 1911, a Royal Medal in 1922 and the Copley Medal in 1935. In 1927 he was awarded a Nobel Prize for Physics with A. H. Compton for their contributions to the understanding of the scattering of high energy photons.

Many details of his early family life and his time at Manchester and the early days at Cambridge have been told in his own words in a manuscript completed ten days before his death in his ninety-first year on 15 November 1959, and published by the Royal Society in *Notes and Records*.

In 1908, at Bellshill, Lanarkshire, C. T. R. Wilson, at the age of 39, married Jessie Fraser Dick, daughter of the Rev. George Hill Dick. They had three children, a boy and then two girls: Mrs Wilson and the three children survived him.

C.T.R.'s scientific work will be described in detail in the subsequent sections dealing with the four main fields of his achievements, which are condensation phenomena, the conductivity of air, atmospheric electricity and the cloud chamber. The invention and perfection of the cloud chamber in 1911 and 1912 was the high point of his long and fruitful scientific life, and by it he first revealed to the eyes of mankind the intimate details of the behaviour of the elementary particles of nature. J. J. Thomson, who communicated all C.T.R.'s early papers to the Royal Society, and to whom Wilson many times expressed his gratitude for support and encouragement, wrote much later in the 'Recollections and Reflections':

'This work of C. T. R. Wilson, proceeding without haste and without rest since 1895, has rarely been equalled as an example of ingenuity, insight, skill in manipulation, unfailing patience and dogged determination. Those who were not working at the Cavendish Laboratory during its progress can hardly realize the amount of work it entailed. For many years he did all the glass-blowing himself, and only those who have tried it know how exasperating glass-blowing can be, and how often when the apparatus is all but finished it breaks and the work has to be begun again. This never seemed to disconcert Wilson; he would take up a fresh piece of glass, perhaps say "Dear, dear", but never anything stronger, and begin again. Old research students when revisiting the Laboratory would say that many things had altered since they went away, but the thing that most vividly brought back old reminiscences was to see C.T.R. glass-blowing. The beautiful photographs that he published required years of unremitting work before they were brought to the standard he obtained. The method has been quickened up and made automatic by other workers, but though they can turn out more photographs in a given time, the photographs themselves are no better than those got by C.T.R. more than twenty years ago. It is to him that we owe the creation and development of a method which has been of inestimable value to the progress of science.'

Condensation nuclei

While holding the Clerk Maxwell Studentship, C. T. R. Wilson made a series of simple and well-designed experiments, which brought clarity and understanding into the hitherto confused situation with regard to the formation of clouds in supersaturated vapours. This work was first described in 1895 and 1896 in two preliminary notes in the *Proceedings of the Cambridge Philosophical Society* and in the *Proceedings of the Royal Society*, followed by three major papers in the *Philosophical Transactions of the Royal Society* entitled: 'Condensation of water vapour in the presence of dust-free air and other gases' (42 pages) in 1897; and in 1899 two papers, 'On the condensation nuclei produced in gases by the action of Röntgen rays, uranium rays, ultra-violet light, and other agents' (50 pages), and 'On the comparative efficiency as condensation nuclei of particles and negatively charged ions' (19 pages). Wilson was then 30 years old.

The major experimental innovation in the first paper consisted in the construction of various ingenious types of apparatus which allowed repeated and very accurately controlled fast expansions of the gas in a glass vessel under conditions of great chemical cleanliness. A reproduction of Wilson's figures describing his first two forms of apparatus has been produced in *Notes and Records*. The first form was of a gasometer type, with the air under investigation contained in an inverted glass vessel dipping into water in a larger container, so that the gas was trapped in the upper part of the vessel. By an ingenious system of valves, taps and subsidiary vessels, the water level in the gasometer could be suddenly lowered, so expanding the gas.

The fundamental discovery made with this apparatus was that once the dust particles, whose role as nuclei of condensation had been studied by Aitken, had been removed from the gas by repeated small expansions, no condensation drops occurred unless the volume expansion ratio exceeded 1·252 in air: the precision of the determination of the volume ratio is typical of his experimentation. The second important result was that for somewhat larger expansions, a few drops were always produced however many times the expansions are repeated. This rain-like condensation must therefore be due to some kind of nuclei which are always present in small numbers, and as fast as they are removed are replaced by others.

Wilson then constructed another much smaller apparatus which allowed greater speed of expansion and still kept the gas from contact with any material but glass and a minimum amount of water. A cylindrical glass bulb was ground to slide as a piston up and down a glass tube, which had a coned constriction at its lower end, against which the piston was ground to fit accurately. By an arrangement of gas and water handling systems, the piston could be made to float at the required height in the glass tube. Then, by suddenly opening a valve at the bottom of the tube, the gas under the piston, which had been adjusted to a suitable pressure in excess of that of the atmosphere, escaped to the atmosphere and the piston moved down very rapidly, in certainly less than 1/100 s, to seat itself on the constriction and so to produce a seal. The construction of the sliding piston and its conical seal in its bottom position was evidently a tricky operation of glass grinding, and Wilson records that many of his pistons shattered on first use. It may well have been in connexion with this apparatus that many stories of Wilson's uncommon patience originated.

With this apparatus Wilson confirmed that no condensation took place in air unless the volume expansion ratio, calculated from pressure measurements, exceeded 1·252, and that this critical ratio did not change appreciably between temperatures of 18 and 28 °C. The number of drops observed was quite small, perhaps of the order of 100/cm^3 and increased rather slowly with expansion ratio until the ratio reached 1·37, when a rapid increase in numbers took place until at 1·38 the condensation no longer gave a rain-like cloud but a fog, taking a minute or more to settle. For still larger expansions, the fog, when suitably illuminated, gave marked diffraction rings which widened as the drops became smaller with increasing expansion. Between expansion ratios of 1·40 and 1·42 there is a very rapid change from blue through violet to red—the violet 'sensitive tint' only existing within a range of 1 or 2 mm difference of initial pressure, the ratio v_2/v_1 being 1·419. Below 1·38 the drops are too few to produce colours, above 1·44 they are too small.

In saturated air, the expansion ratio to produce a fog-like condensation, as contrasted with the rain-like condensation, began at an expansion ratio of 1·37 to 1·38. Nearly identical results were found with oxygen and nitrogen, but in hydrogen, while the fog limit was the same, there were so few rain-like drops for lower expansion ratios that Wilson could not define a lower limit accurately. This was mainly due to the small size of the apparatus. Carbon dioxide gave values of 1·36 for rain-like condensation and 1·53 for fog.

Wilson proceeded to calculate the supersaturation attained at the end of the adiabatic expansion, when the initially saturated gas reached its lowest temperature; the supersaturation was defined as the ratio of the actual vapour pressure π_1 to the equilibrium vapour pressure π_2 over a flat surface at the lowest temperature, and was given by

$$S = \frac{\pi_1}{\pi_2}\left(\frac{v_1}{v_2}\right)^{\gamma}$$

where γ is the ratio of the specific heats of the gas and v_1 and v_2 are the initial and final volume. In air, N_2 and O_2 ($\gamma = 1{\cdot}41$), the rain-like cloud limit $v_1/v_2 = 1{\cdot}252$ corresponds to a supersaturation of 4·2, and the fog limit of 1·38 corresponds to a supersaturation 7·9. In CO_2 ($\gamma = 1{\cdot}31$) the two expansion limits were 1·36 and 1·56 respectively, but the calculated supersaturations were almost unchanged.

Wilson calculates the number of drops in the cloud obtained with $v_2/v_1 = 1{\cdot}42$ by assuming from their optical properties that they must be somewhat smaller than the wavelength of light in the brightest part of the spectrum, and so he takes their diameter to be 5×10^{-5} cm. Their volume is then divided into the volume of water that must condense out of the vapour by the end of the expansion; he found in this way that there must be about 10^8 droplets per cm^3.

The last part of the paper describes the effect of X-rays on the condensation phenomena. It is shown that for $v_1/v_2 < 1{\cdot}252$ no cloud is produced, but for larger expansions a huge number of drops are formed so as to give a slowly falling fog. Further, if the X-rays were switched off half a minute before the expansion was made, no condensation was found—other than the normal few rain-like drops. The nuclei produced by X-rays thus required exactly the same supersaturation as those occurring spontaneously in normal air: moreover, they disappeared completely in less than half a minute.

By using Kelvin's expression for the equilibrium vapour pressure over a curved surface, and by assuming for simplicity that the surface tension T remains the same for very small drops, which he knew from the properties of thin films not to be strictly the case, Wilson calculated the radius of the drops as

$$r = \frac{2T}{R\theta \log_e S}$$

where R is the gas constant and θ is the lowest temperature reached. The radius of a drop just large enough to grow in vapour supersaturated to the extent required to make rain-like condensation in air, is found to be $8 \cdot 6 \times 10^{-8}$ cm, while the size to make drops grow when the supersaturation is enough to make a dense cloud is $6 \cdot 4 \times 10^{-8}$ cm. Wilson considers that the vast numbers, of the order of 10^8 per cm^3, of the latter type of nuclei can be none other than simple aggregations of water molecules such as may come together momentarily through random encounters of water molecules. He ends this paper as follows: 'The nuclei which bring about rain-like condensation, and the greater number of which appear to be equivalent in their power of causing condensation to water drops of not much less than $8 \cdot 7 \times 10^{-8}$ cm, are probably of a different character. As, however, I am continuing these experiments, it would be premature at the present stage to discuss the various views that might be held as to their nature.'

Nowhere in this first long paper are the nuclei identified with charged ions, but in a subsequent short paper in the *Proceedings of the Cambridge Philosophical Society* (**9**, 333, 1897) Wilson definitely identified the nuclei produced by both X-rays and the rays from uranium with the nuclei which are always present in small numbers and which produced the rain-like condensation in clean air: for all these required identical expansions.

Since the electrical properties of gases exposed to these rays showed the presence of free ions, it became highly probable that the nuclei were in fact free ions, and Wilson used a theoretical result by J. J. Thomson, published in 1893, to show that the electric charge of a free ion should encourage the process of condensation.

The systematic continuation of this experiment was described in 1899 in the second of his three big papers in the *Philosophical Transactions of the Royal Society*, and was directed to the study of the efficiency as nuclei of condensation of the carriers of electricity in gases, when these are made in different ways. With his usual concern for verbal clarity, he explains that he will talk of the expansion required to 'catch' nuclei as meaning the expansion required to cause water to condense on them: the terms 'larger' and 'smaller' are used of nuclei to denote that they require smaller or larger degrees of supersaturation respectively—a nucleus is said to grow when it becomes larger in this special sense. Wilson suggests that the terms 'larger' and 'smaller' are probably to be taken literally, since the nuclei are most likely very small drops of water which are able to persist in spite of their small size, because the effect of the curvature of their surface in raising the equilibrium vapour pressure is balanced by the opposite effect due to the drop being charged electrically or due to it containing some substance in solution.

A number of different forms of apparatus were constructed to make various types of observations and measurements; all were simple and very suitable for their purpose. The results with X-rays reported in the previous paper were confirmed by the additional observations that prolonged exposure to the rays does not cause the nuclei to grow: for instance, a 10 s exposure followed by an expansion with $v_2/v_1 = 1{\cdot}271$ produces a dense fog, but a 10 min exposure followed by an expansion of 1·245 produces no drops at all. The rapid decrease in the numbers of drops when the expansion is made some seconds after the exposure is attributed to the recombination of the ions. Uranium rays and X-rays were shown to produce identical types of condensation nuclei, as had already been reported in the short earlier paper.

Then comes a long section dealing with the nuclei produced by ultra-violet light from an electric spark admitted through a quartz window, this extending the earlier work of Lenard and Wolff. It is shown that these nuclei grow larger with larger exposure and greater intensity of the radiation, and in extreme cases form without any expansion at all: they are thus essentially different from ions. A number of different experiments were made which prove that the nuclei are produced in the volume of the gas and not, as thought by some workers previously, by the ejection of particles from the walls of the vessel; it is also proved that pure oxygen and water are alone needed for this production. The suggestion is made that under the influence of the ultra-violet rays, H_2O_2 is formed which dissolves in the droplets and lowers the vapour pressure, so allowing them to grow.

The effect of sunlight is then investigated and it is found that nuclei are produced when the rays have to pass through a blue glass screen but not through a red one. It is suggested that sunlight might be able to produce large enough nuclei in the atmosphere to produce drops with very small or no supersaturation and that these might be the particles which produce the scattering resulting in the blue colour of the sky (see *Proc. Camb. Phil. Soc.* **9,** 392, 1897). The effect of metals was then studied, and a fresh metal zinc surface, especially when amalgamated with mercury, was found to produce condensation nuclei. Condensation nuclei were also found when an electrically charged platinum wire was used, but only when the point of the wire was luminous when viewed in the dark.

The final proof that the condensation nuclei produced by X-rays and uranium rays consist of ions was obtained by applying an electric field across the expansion space, when it was found that the fog, produced in the absence of the field, disappeared. Rutherford's measurements of the mobility of ions was used to calculate that ions would be swept away by the field of 150 volts/cm in less than 1/100 s—this fully explained the observation. On the other hand, an electric field produced no reduction of the fog caused by ultra-violet light, thus showing that the nuclei were uncharged.

In the last section of the paper, Wilson calculates the approximate value of the electric charge e which would balance the effect of the curved surface and so prevent the droplet from evaporating. Making use of an expression obtained by J. J. Thomson, he obtained the relation

$$e^2 = 16\ Ta^3$$

where T is the surface tension and a the radius. Putting $a = 8 \cdot 6 \times 10^{-8}$ cm, as obtained in the previous paper, e is found to be $1 \cdot 5 \times 10^{-9}$ e.s.u. which is, of course, about three times the actual electronic charge.

Wilson's third long paper (*Phil. Trans.* A, **193,** 289, 1899) was communicated to the Royal Society by the Meteorological Council, and was concerned with the difference in efficiency of positive and negative ions as condensation nuclei. He mentions that J. J. Thomson had pointed out in the *Philosophical Magazine* (**46,** 528, 1898) that if such a difference existed, then one might find in the atmosphere clouds of water drops condensed on ions of one sign only. Then the fall of these drops under gravity would separate positive and negative electricity and so provide a possible mechanism for the field in a thunder cloud.

Wilson constructed a horizontal and shallow cylindrical chamber with metal roof and floor and passed through it a narrow beam of X-rays very close to the lower plate. If the electric field between the plates was upwards, then positive ions would move upwards into the main part of the chamber, while the negative ions would move downwards into the plate, so allowing the behaviour of a cloud of positive ions to be studied; by reversing the field, the chamber would contain negative ions. In this way the expansion required to condense water or positive ions was found to be greater than in negative ions, thus making it clear that all the previous experiments on the least expansion to make condensation take place on ions had been concerned only with the negative ions.

Wilson had previously suspected an increase in the number of drops at an expansion of about 1·31, and states that J. J. Thomson had been led to consider that there might be a difference in behaviour of positive and negative ions by himself noticing such an increase. Wilson argued that the difference in supersaturation could not be due to the negative ions having, say, twice the charge, because equal numbers of positive and negative charges are produced when a gas is ionized.

Wilson then attempted to answer the question of whether ions are likely to be present in the atmosphere under normal conditions. The identity of the expansion limits for rain-like condensation and that required for condensation on ions strongly suggested that they were. Moreover, the number of these nuclei was too small to make the absence of a hitherto detectable electrical conductivity in air under ordinary conditions inconsistent with the view that they were ions.

Wilson reported, however, that all attempts to remove these nuclei by a strong electric field failed even when using a sensitive differential apparatus, and concludes his paper with the following statement:

'Such nuclei, therefore, in spite of their identity as condensation nuclei with the ions, cannot be regarded as free ions, unless we suppose the ionization to be developed by the process of producing the supersaturation. The question requires further investigation.'

He soon proved this conclusion wrong, for two years later, in 1901, in a paper entitled 'On the ionization of atmospheric air', he produced evidence that the lack of an apparent observed effect of an electric field was probably due to the small number of nuclei present. He used his experimental proof of the spontaneous ionization of air (described in his paper in the *Proceedings of the Royal Society* in 1901) to identify the nuclei of the rain-like condensation as being due to these ions.

Three years later Wilson returned to this work in greater detail. This was in a paper entitled 'The condensation method of demonstrating the ionization of air under normal conditions' (*Phil. Mag.* **7,** 681, 1904), in which he notes the absolute identity of the supersaturation required to condense rain-like drops on the nuclei always present in a gas, and those produced by an ionizing agent. He contrasts this with the lack of any reduction by means of a strong electric field of the former type of droplet but the very big reduction of the latter type. He concludes that his earlier suggestions that the nuclei might be produced by the expansion was probably wrong and that his experimental observations of the lack of influence of electric field on the number of drops must have been at fault, due to the too small size of apparatus, which meant very few observed drops. For since the former paper, he himself and Geitel independently had demonstrated the electrical conductivity of normal air, so that ions were definitely present. So a much larger apparatus was built, which incidentally was a major step towards the perfected cloud chamber of 1911.

The chamber in which the drops were formed consisted of a glass cylinder 18·5 cm in diameter and 6 cm high, sealed on the top by a brass disk and with its bottom resting on a rubber ring lying on an annular brass plate. A brass expansion cylinder 30 cm long and 10 cm in diameter was attached to the annular plate and contained a light floating brass piston with hemispherical top, which was lubricated and made gas-tight by the tube being nearly filled with water. By suddenly connecting the space under the floating piston to an evacuated vessel by means of a valve made out of a rubber cork, the piston could be suddenly lowered, so producing the required expansion in the shallow glass cylinder above.

With this apparatus it was found that a potential difference of 160 volts between the top and bottom of the chamber did make a big reduction in the number of rain-like drops: in fact, a potential difference of only 2 volts caused a noticeable reduction. It was concluded that in the earlier small chamber accidental differences of potential could have been sufficient to reduce the number of drops so much that the application of larger fields was not noticeable.

Wilson calculated the number N of ions in a vessel of height l, filled with a gas for which the ionic diffusion coefficient is D, to be $ql^2/3D$ where q is the rate of production of ions, and when no electric field is used. Using the values of q obtained by Cooke, about 13 ions of either sign produced per second in each ml., Wilson gets about a predicted value of 1000 for N in his large chamber. He states that he did not succeed in making a direct determination of the numbers, but made an indirect one, by the method used by J. J. Thomson to determine the ionic charge.

He pointed out that the total number of ions present when a steady state is reached, in the absence of an electric field, is such that the number removed by recombination and diffusion to the walls is equal to the number produced per second: in his experiments diffusion is shown to be more important than recombination, because of the small number of ions present. The effect of the electric field is just to sweep away the ions so as to reduce the number available to act as nuclei when the expansion is made.

The size of each drop is calculated from the rate of fall, using Stokes's formula, and the total mass of water condensed from the condition of an adiabatic expansion; assuming this is equally distributed among the drops, their number follows and was found to be in rough agreement with the previous estimate.

These simple but revealing calculations are a marked characteristic of Wilson's methods: he wanted always to understand quantitatively just what physical processes went on in his experimental apparatus—he usually succeeded.

Two other publications on condensation nuclei appeared in 1904, one being a report of a lecture at the Royal Institution on 19 February, in which he evidently showed many ingenious demonstration experiments, and the other a paper to the International Electrical Congress at St Louis, U.S.A. In the latter Wilson gives his most complete survey of the physics of condensation on nuclei. He extends Kelvin's well-known derivation of the difference of vapour pressure over a curved and a plane surface, as derived from the depression of a column of liquid in a capillary to much smaller drop sizes, obtaining the relation

$$\log_e \frac{P_2}{P_1} = \frac{1}{Rt}\,\frac{2T}{r}$$

where P_2 is the pressure of water vapour in equilibrium with a drop of radius r and surface tension T at temperature t and P_1 is the corresponding pressure for a flat surface. This equilibrium is clearly unstable: if a drop gets too big, it will grow; if too small, it will evaporate completely.

Wilson now gives J. J. Thomson's result for the effect of a surface electric charge, by calculating the change of height of the column of liquid due to the electric field producing the surface charge:

$$\log_e \frac{P_2}{P_1} = \frac{1}{Rt}\left(\frac{2T}{r} - \frac{e^2}{8\pi r^4}\right)$$

where e is the charge on the drop. The maximum vapour pressure occurs for $r^2 = e^2/4\pi T$ and has the value given by

$$\log_e \frac{P_2}{P_1} = \frac{3T}{2Rtr}$$

If the vapour pressure is increased above this value, an unstable condition results and the drop continues to grow so long as the supply of vapour is unlimited.

I still possess a much-thumbed reprint of this paper which, in 1921 when I started working on the cloud chamber, was the best account of the fundamental condensation phenomena underlying its operation. The application of these expressions to condensation phenomena ends by listing the three principal types of ions: '(1) The ions proper, requiring a four-fold or six-fold supersaturation to cause water to condense on them, and having a mobility exceeding 1 cm per second in a field of 1 volt per cm; (2) loaded ions, requiring little or no supersaturation to make water condense on them, and having a mobility generally less than a thousandth part of that of the ions proper; (3) uncharged nuclei, resembling the second class in requiring little or no supersaturation in order that visible drops may form on them.'

In 1903 Wilson reviewed critically in *Nature* a book by Barus on condensation phenomena, to which Barus replied with obvious annoyance. Wilson's comments on the reply had a caustic quality, of which those who only knew him in later life would not have expected him to be capable.

The conductivity of air

C.T.R.'s crucial discovery of the spontaneous ionization of air arose directly out of his attempts to explain the rain-like condensation observed when no ionizing sources were used; it was reported first in a short note in the *Proceedings of the Cambridge Philosophical Society*, 1900, entitled 'On the leakage of electricity through dust-free air'. In this paper he refers to the nearly simultaneous but quite independent discovery by Elster and Geitel (*Phys. Z.* **2,** 116, 1900) of the conductivity of air. Fuller details were given the next year in 'On the ionization of atmospheric air' in *Proc. Roy. Soc.* **68,** 151, 1901). The essential innovation was the complete avoidance of any leakage of electricity through the insulated supports of a gold-leaf electroscope by the following means. A sulphur bead was used to attach the gold-leaf system to a conducting rod which was kept at the initial potential of the leaf: thus any continuous fall of the leaf was necessarily due to leakage through the air and, moreover, if the insulator were imperfect, it would reduce and not increase the observed effect.

With this apparatus he showed that the leakage was the same in daylight and in the dark, and whether the charge was positive or negative, and whether the initial potential was 120 or 210 volts. All these three conclusions had also been arrived at by Geitel. Wilson added two more: that the rate of leak is proportional to the pressure and that the rate is equivalent to the production of about 20 ions of either sign in each cm^3 per second at atmospheric pressure. After giving the details of these measurements, Wilson, without any prelude, makes the laconic but historic statement: 'Experiments were now carried out to test whether the production of ions in dust-free air could be explained as being due to radiation from sources outside our atmosphere, possibly radiation like Röntgen rays or like cathode rays, but of enormously greater penetrating power.' A portable form of electroscope was made and taken at night into the Caledonian Railway tunnel near Peebles, when it was found that the rate of leak was the same, within the experimental error, as in the open. Wilson, concludes: 'It is unlikely, therefore, that the ionization is due to radiation which has traversed our atmosphere; it seems to be, as Geitel concludes, a property of air itself.' This historic experiment is rightly held by historians to be the beginning of the great and fertile subject of cosmic rays. However, it was not until 1912 that the extra-terrestrial origin of these rays was finally proved by Hess.

An account of the origin of the hypothesis that very penetrating rays might be reaching the earth from outside the atmosphere is to be found in a paper reviewing the state of knowledge of atmospheric electricity published two years later in *Nature* (**68,** 102, 1903). In this paper Wilson discusses the difficulty of understanding how the earth's negative electric charge could be maintained, especially in view of the recently discovered conductivity of the atmosphere, and writes as follows:

> 'It is quite conceivable that we may be driven to seek an extra-terrestrial source for the negative charge of the earth's surface. The study of the aurora borealis has led several observers to the conclusion that the sun emits kathode rays, which are deflected by the earth's magnetic field, and travel in helical paths round the magnetic lines of force towards the poles. It is conceivable that very penetrating rays of this type (i.e. negatively charged electrons) may traverse our atmosphere unobserved, and be stopped in the solid mass of the earth, giving to it their negative charge.'

As often in the history of science, a plausible hypothesis, even though later proved to be false, has provided a valuable stimulus to further experiment and so to important discovery. The earth is being bombarded by energetic particles of extra-terrestrial origin. But they are mainly positively charged and are not the cause of the earth's negative charge.

In a second paper in 1901 (*Proc. Roy. Soc.* **69,** 277) Wilson showed that the leakage current in different gases was nearly proportional to the density (hydrogen excepted) and, moreover, the relative conductivity (air = 1) was the same for polonium α-rays and for penetrating radium γ-rays. He concludes: 'Until, however further experiments have been made it would, I think, be premature to conclude that the ionization is due to radiation from the walls of the vessel.'

After a short paper on the radioactivity carried down by rain and snow, Wilson in 1903 gave an account of his well-known tilted electrometer. This depended for its sensitivity on the use of an auxiliary electrode disposed and charged in such a manner as to allow the gold-leaf to be brought to a state of near instability. By this ingenious trick Wilson raised the sensitivity by a factor of 100. The paper ends with a remark which shows C.T.R.'s constant concern for the simple and practical: 'The instrument may be carried about without risk of injury to the gold-leaf; it is only necessary to charge up the plate to such a potential that the gold-leaf (connected to the case) is stretched straight out towards the plate, which it is just too short to touch. The apparatus may then safely be inverted or carried in any position; the gold-leaf remains steadily pointing towards the plate.'

C. T. R. Wilson's main interest in the next few years, that is, after about 1904, lay in the field of atmospheric electricity and thunderstorms, and it was not until about 1910 that he again worked actively at condensation phenomena—to produce the cloud chamber in 1911. In the next section Wilson's work on atmospheric electricity, which continued right up to his death, will be discussed.

Atmospheric electricity and thunderstorms

As has been related, Wilson's interest in atmospheric electrical phenomena arose from his own experiences as a youth on a mountain in a storm and he continued to study the subject all his working life. His last scientific paper, 'A theory of thundercloud electricity', was communicated to the Royal Society in 1956 in his eighty-seventh year, when he was the oldest Fellow. It is believed that not for three hundred years has the oldest Fellow communicated a paper to the Society.

As mentioned above, Wilson wrote a survey of the state of knowledge of atmospheric electricity in 1903. Then in 1906 he published his first major paper on atmospheric electricity entitled 'On the measurement of the earth-air current and on the origin of atmospheric electricity'. He describes observations of the earth-air current by a novel and simple method, which can best be described in his own lucid words:

> 'An insulated conductor connected to an electrometer is initially at zero potential and under a metal cover. The earth connexion is broken and the cover removed, the conductor being thus exposed to the earth's electric field. The potential of the conductor is thus raised, but is at once brought back to zero by means of a compensator. When this adjustment has been made, we know that the charge removed from the electrometer and its connexions by

the displacement of the compensator is equal and opposite to that held on the exposed part of the conductor when at zero potential under the action of the earth's field. When the compensator has once been standardized, its readings measure the charge on the exposed conductor when kept at zero potential; this charge will be the same as if the conductor were earth-connected. If now by means of the compensator the conductor be maintained at zero potential for a few minutes and the cover be then replaced, the new reading of the compensator, when again adjusted to bring the electrometer reading back to its zero, measures the charge which has entered the conductor from the atmosphere in the given time.'

With this apparatus erected on the top of Hamildon Hill near Peebles in Scotland, Wilson found that the average dissipation of the electric charge on the exposed conductor was about 5 per cent per minute, but varied considerably with the weather conditions. He investigated the charge on and the current through a growing plant in a flower-pot placed on the test plate. No marked difference was found for the dissipation rate for the plant and for the test plate alone. In a theoretical note at the end of the paper the maintenance of the normal electric gradient in the atmosphere is discussed, and it is held to be probable that the negative charge carried down by rain is possibly sufficient to balance the positive charge flowing downwards into the earth due to the positive potential gradient and the conductivity of the air. The possibility, already discussed in 1903, that the maintenance of part of the negative charge on the earth's surface might be due to a penetrating radiation reaching the earth's surface from cosmical sources, is again mentioned.

A continuation of these experiments was reported in a paper in 1908 in the *Proceedings of the Royal Society*. Among many results Wilson showed that the dissipation was not greater in bright sunlight, as would be expected if the loss of negative charge of the test plate had been due to a photoelectric effect. The electric charge on growing turf was always about three times that on the bare test plate but the leakage was also about equally greater, so giving the same rate of dissipation. In the case of the turf the charge was concentrated on the tips of the leaves. Wilson concludes that these experiments demonstrated that the fraction of the charge per unit area of the ground which is neutralized per minute is the same as that found for the test plate of the apparatus. It thus became useful to calibrate the instrument in absolute units, by comparing the charge and current measured with the original small test plate, which stood well above the ground, with the current and charge for a larger test plate placed with its upper surface very little raised above the surface of the ground. For a number of days in 1906 and 1907 Wilson found that the mean downward electric current was $2 \cdot 22 \times 10^{-16}$ amp per cm^2, which was in close agreement with some measurements by Gerdien in 1905 of the conductivity and the potential gradient. The average charge on the surface was found to be $16 \cdot 6 \times 10^{-14}$ coulombs per cm^2 and the corresponding potential gradient was about 200 volts per metre. The dissipation factor was least (6·6 per cent) for the calm and cloudless days, and greatest (11·2 per cent) for days with cumulus clouds.

In 1916 and 1920 Wilson published important papers on the determination of the sign and magnitude of the electric discharge in lightning flashes. He starts by pointing out that only the roughest estimate of the charge in a lightning flash then existed. Schuster had used two determinations by Pockels of the maximum current as measured by its magnetizing effect, and assumed that the duration was 1/1000 second and so found that the charge brought down was about 10 coulombs. Wilson set out to measure the change of the vertical electric field at the surface of the earth when a lightning flash occurs. If a charge Q at a height H passes to earth, the change of the vertical field at a distance R will be $2QH/R^3$. The apparatus was a development of that already described and consisted essentially of an insulated circular conductor, 59 cm in diameter, placed in a pit in the ground near the Solar Physics Observatory, Cambridge, with its upper surface level with the ground. The conductor was connected to earth through an electrometer of a special type developed by Wilson from Lippmann's design. Whenever the charge on the conductor changed so as to raise its potential above that of the earth, the mercury column in the capillary moved in such a way as to bring the potential back to zero, while the displacement of the mercury measured quantitatively the amount of charge received by the conducting plate. An earthed cover normally screened the plate from the earth's electric field. When this was removed so as to expose the plate, the plate acquired a charge, and the amount of this was measured by the displacement of the capillary electrometer. When a lightning flash occurred, the change of the vertical electric field was determined by the displacement of the mercury column of the electrometer, and, together with the distance R of the storm (as obtained from the time from flash to thunder), gives the product HQ, that is, the electric moment destroyed by the flash.

Wilson found it possible to determine H and so Q separately by measuring the change of field with distance from nearby storms. For the field change due to a flash will have the opposite sign directly under the storm to that at a large distance, and there will be a critical distance at which the field change is zero. This method depended in practice on the observation that successive flashes from certain storms were very similar, so that measurements of the change of field from successive flashes from the same storm at different distances were equivalent to measurements of the same flash at different distances. By such means Wilson deduced that in the majority of storms there was an upper positively charged region and a lower negatively charged one, and also that the direction of the currents in the lightning flashes was upwards, that is, in the opposite direction to the fair weather atmospheric current. In the important 1920 paper, Wilson first made the definite suggestion that the electric fields and currents of fine weather could be maintained by the currents in storms and showers. The average quantity of electricity discharged at each flash was found to be close to 30 coulombs, but the distance between positive and negative charges before the flash was much less well determined and could have lain between 1 and 10 kilometres.

These pioneer results have been in general fully confirmed. Important factors in this achievement were Wilson's flair for designing simple but effective apparatus based on a clear understanding of the physical principles involved, and on a deep interest in natural phenomena.

In 1925 Wilson wrote a paper entitled 'The electric field of a thunderstorm and some of its effects'. Assuming that a thundercloud is an electric generator in which the separation of positive and negative charges occurs at a rate which corresponds to a current of some amperes and in which the potential difference between its poles amounts to about 10^9 V, the following effects are discussed. The electric field above the cloud should be sufficient to initiate a continuous or discontinuous discharge between the top of the cloud and the upper atmosphere at about some 60 km height, and this may be one of the causes of atmospherics arising from regions of rain unaccompanied by thunder. Then the electric field at the earth below a thundercloud may often exceed 10000 V/cm, and this is quite sufficient to produce a brush discharge from an elevated or pointed conductor, as, of course, has been known from the phenomenon of St Elmo's fire. Wilson had earlier made some experiments to find the potential gradient over grass-covered ground which would produce a measurable current, and found that above fields of 15000 V/m the current becomes quite large, of the order of 1 A/km^2. Lastly he refers to his paper of the previous year which showed that fast β-particles already present in the atmosphere should be accelerated by the strong electric fields associated with a thunder cloud to energies of 10^9 eV, that is, comparable to the energies of some cosmic rays.

In this former paper (published in *Proc. Camb. Phil. Soc.* 1925) Wilson drew attention to the decrease in the energy loss of a β-particle with increasing energy, so that there must be for any gas a given electric field above which a β-particle moving in the direction of the field will gain more energy than it will lose. In air at atmospheric pressure, a 20000 volt electron loses 10000 V/cm, so in a field of 20000 V/cm, it will gain energy at the rate of 10000 eV/cm. The effect of nuclear collisions in stopping the gain of energy by deflecting the particles is discussed in detail. Since a very fast particle only loses 1000 eV/cm, it would be accelerated in a field greater than this if moving in the direction of the field. It would still be accelerated even if initially moving within an angle only a few degrees short of 90° in a field of 20000 V/cm. From the amount of radium emanation in the atmosphere Wilson calculates that the number of β-particles emitted which would be capable of being accelerated should be about 10 s^{-1}m^{-3}.

In 1929 Wilson published in the *Journal of the Franklin Institute* a paper entitled 'Some thunderstorm problems', in which he first outlined his ideas on the mechanism of thunderstorm activity. 'According to this a thundercloud is essentially bipolar, the positive charge tending to be above the negative. The preponderance of negative potential gradients below shower clouds and thunderclouds is on this view due primarily to the negative charge of the cloud being nearer the ground than the positive; the cloud may in addition acquire an excess of negative charge when the loss of positive charge by conduction to the upper atmosphere exceeds the loss of negative charge to the ground. The prevailing positive charge on rain is on this view not the cause but the result of the negative potential gradient; the rain intercepts and returns to the earth a portion of the charge carried by the stream of positive ions which are being driven up by the negative potential gradient.

'There would probably be general agreement that the source of the electromotive force of a thundercloud is to be sought in the vertical separation under gravity of carriers of positive and negative electricity. Let us suppose that the small particles in a cloud, which only fall slowly relatively to the air, are positively charged, while the larger drops which fall with considerable velocity through the air are negatively charged. We may leave for the present the question how the carriers acquire their charges.

'Such a cloud, originally neutral, will at once begin to acquire a positive charge at the top and a negative charge at the bottom through the relative vertical motion of the two classes of carriers. The two equal and opposite charges thus accumulating at the top and bottom may be separated by a great thickness of neutral cloud. The accumulation of a positive charge above and a negative charge below results in the development of an electric field within the cloud, which tends to hinder the negative drops from falling and the positive particles from being carried up by the air stream.'

Wilson's last paper, 'A theory of thundercloud electricity' in the *Proceedings of the Royal Society* in 1956, includes work which had occupied him for very many years. The best summary is his own: 'The thundercloud is regarded as a great influence machine, the ionization currents associated with it being the agents by which its electromotive force is developed and maintained. The moving ions which constitute these currents may be intercepted by solid or liquid cloud elements so that it becomes possible for them to be carried against the field and so increase an existing electromotive force. The early stages in its growth are due to the ionization current within the cloud initiated by the earth's fine weather field. Later it is the external currents due to the thundercloud's own field which are effective. Lightning discharges may themselves contribute to the electromotive force of the thundercloud.' The argument is very detailed and reveals the deep knowledge which Wilson had acquired by many years of thought on the physical processes which must occur within a thundercloud. The main argument concerns the way in which falling water droplets of various sizes, and so with different rates of fall, acquire electric charges by collision with positive and negative ions moving in the existing vertical electric field. A falling drop is polarized by the field so that its lower half is positively charged, and so selectively attracts negative ions and thus acquires a net negative charge. As the drop falls towards the bottom of the cloud it thus augments the pre-existing downward-directed electric field—so making possible the building up of very large potential differences between the top and bottom of the cloud.

Though Wilson's mechanism must certainly operate, it is now believed that it cannot by itself produce a rapid enough separation of charge. Many other mechanisms have been suggested, but there is as yet no generally accepted mechanism of the charge separation in a thundercloud. It seems rather likely that the charge separation may occur in some rather complicated way during the collision of supercooled water drops with ice crystals.

Whatever explanation of the remarkable natural phenomenon of the thundercloud becomes finally accepted, there is no doubt that Wilson's pioneer work, with both its elegant experimentation and its deep physical thought, will remain a major contribution to its final understanding.

The cloud chamber

In his Nobel Lecture in 1927 Wilson states that after his experiments published in 1904, which proved that the nuclei producing the rain-like condensation are removable by an electric field, he did not resume his work on condensation phenomena until about 1910. As has been mentioned, these intervening years were mainly occupied with the investigation of atmospheric electrical phenomena. Moreover, these years had seen much more definite ideas develop on the nature of radioactive rays and X-rays, so that Wilson considered seriously the possibility of making the track of an ionizing particle visible by photographing the drops of water condensed on the ions in its track. He had also in mind developing a method of measuring the electronic charge by counting the number of drops produced by a given total measured electric charge; however, his own success with making tracks visible and Millikan's in the development of his oil drop method led him to abandon this.

The first photographs of the tracks of atomic particles were obtained in the spring of 1911 with a relatively rough apparatus and were published in the *Proceedings of the Royal Society* (A, **85,** 285, 1911). The main difference from the 1904 apparatus was in the shape of the expansion chamber itself, which was 7·5 cm in diameter, had a flat roof and floor and an expanded height of 6 mm. The experiment was made just as in 1904 by the sudden downward displacement of the floor which constituted the top of the light piston. The flat shape served to prevent the stirring of the gas on making an expansion.

The use of a moist gelatine layer on the glass surface was introduced, both to prevent the condensation of dew on the glass and to provide a conducting layer for the application of an electric field. To photograph the tracks, a condenser discharge through a capillary of mercury vapour at atmospheric pressure was used. Two photographs were published, one showing quite clearly a few α-ray tracks from a weak radium source and the other the short tracks of electrons due to the passage of an X-ray beam. Using a 30 mg radium bromide source, the tracks of many fast β-particles were observed, but no photograph was reproduced.

Wilson comments: 'Whether the original X-radiation has a continuous wave front, or is itself corpuscular as W. H. Bragg supposes, or has in some other way its energy located around definite points in the manner suggested by J. J. Thomson, remains undecided.' This remark clearly expresses the then still not quite clear concept of the photon structure of radiation.

In the following year Wilson published a detailed account of a new, larger and much improved cloud chamber (*Proc. Roy. Soc.* A, **87,** 277, 1912), with many exquisite photographs of α-, β- and X-ray tracks—they still remain among the technically best photographs ever made. The cloud chamber itself had taken on its final form, to remain for two decades a standard instrument of great simplicity and superb performance. Wilson never made another of this type, but used the same one right on into the 1920s.

The construction of the cloud chamber is so well known as hardly to need detailed description: it was 16·5 cm in diameter and 3·4 cm deep, with glass roof and glass floor fixed to the top of a thin-walled brass piston sliding freely in an outer bronze cylinder, and made gas-tight by a water seal. The expansion was made by connecting the space under the piston with an evacuated vessel using a rubber valve as in earlier experiments. Essentially the same type of capillary mercury vapour spark actuated by Leyden jars was used to provide the illumination. A simple arrangement of a single falling weight was used first to open the valve, then to energize the X-ray tube and finally to make the spark to illuminate the chamber. Photographs were taken with a stereoscopic camera.

Very fine photographs of α-rays, both from a minute amount of radium on the tip of a wire and from radium emanation in the gas, were reproduced. Several α-rays near the end of their range showed sudden changes of direction due to collisions with the nuclei of the atoms of air; in some cases the short track of the recoiling nucleus was clearly visible. It was the year before, 1911, that Rutherford had deduced the existence of the atomic nucleus from the observed large angle of scattering of α-particles.

The tracks of fast β-particles were also shown, with the individual droplets due to single ions clearly visible. Then came some exquisite photographs of the secondary electron tracks produced by a narrow beam of X-rays. Wilson emphasizes that there is no indication of any effect of the X-rays other than by the production of corpuscular radiation, thus confirming W. H. Bragg's view that ionization by X-rays is entirely a secondary process. He also notes that the secondary ionizing tracks (clearly electrons) show two kinds of deflexions as a result of their encounter with atoms of the gas—Rutherford's single and compound scattering. By flashing on the X-rays before the expansion, broad diffused tracks were obtained, allowing the number of positive and negative ions to be separately counted.

The impact on the scientific world of these wonderful photographs of atomic tracks was profound. Phenomena which had been deduced indirectly from electrical measurements and perhaps only dimly envisaged now became visible to the eye in all their finest details. Wilson had given the world a new sense. There are many decisive experiments in the history of physics which, if they had not been made when they were made, would surely have been made not much later by someone else. This might not have been true of Wilson's discovery of the cloud method. In spite of its essential simplicity, the road to its final achievement was long and arduous: without C. T. R. Wilson's vision and superb experimental skill, mankind might have had to wait many years before someone else found the way.

As was to be expected, Wilson became much in demand as a lecturer, and this resulted in two further accounts of his work covering essentially the same field being published in 1913: one lecture being given to the Royal Institution on 7 March 1913 and the other to the Société française de Physique on 18 March (published in the *Journal de physique*). These papers cover essentially the same ground as the 1912 paper in the *Proceedings of the Royal Society*, but include for the first time photographs of a narrow X-ray beam traversing the chamber and being partially absorbed in a thin metal target in the chamber.

It was not until after the end of the 1914-18 War that Wilson was able to continue this work, using the same cloud chamber—the only one of its type he had built, but with many improvements in detail. Wilson had moved from the Cavendish Laboratory to the Solar Physics Observatory on the Madingley Road, and all his subsequent work was carried out there.

Between December 1921 and July 1922 Wilson took 500 stereoscopic pairs of photographs and published the results in two big papers in *Proceedings of the Royal Society* A, **104,** 1, 192, 1923: 'Investigations on X-rays and β-rays by the cloud method. Part I: X-rays, Part II: β-rays.' In the first paper 22 superb photographs of the secondary electron tracks produced by X-rays were reproduced; many of them, along with some of the photographs from the 1912 paper, have been reproduced over and over again in textbooks of physics. The perfection of the technique is exceptional, the tracks are in perfect definition, the background is quite black (signifying a high degree of cleanliness of the glass surface), and water drops other than those on ions are almost completely absent.

The object of the experiment was to understand the detailed mechanism of the absorption of X-rays, in both gas and metal targets placed in the chamber. To this end a careful study was made of the visual appearance of the electron tracks, which were of three types: (*a*) long tracks with a range of several centimetres, (*b*) small spherical cloudlets, and (*c*) tracks of 1 to 2 mm range with the initial direction coinciding with the direction of the X-ray beam—Wilson called these 'fish tracks'. The long tracks were identified as electrons ejected from an atom by one quantum of radiation, that is, photoelectrons. On the other hand, the fish tracks, though certainly due to the direct action of the X-rays, were produced by an electron with an energy which was very small compared with the quantum of the primary radiation. Wilson related the tracks specifically to the scattering process suggested by

A. H. Compton, in which the recoil electron takes up the momentum of the quantum of radiation. The spherical tracks were held to be mainly the tracks of electrons of energy less than about 2000 V; these were explained as being mainly due to secondary quanta of radiation produced by atoms excited by the primary quanta. A statistical analysis of the directions of emission of the photoelectrons was made: the predominantly forward direction is well shown in the exceptionally beautiful photograph shown in his figure 6, and the occurrence of both forward- and backward-directed tracks in figure 7.

Detailed studies were made of the various types of associated tracks, in particular paired tracks, and most of these were related to the action of secondary quanta arising from atoms from which the *K* and *L* electrons had been ejected by the incident quantum. In his summary of his results, Wilson remarks: 'The cloud method is able to deal with individual quanta of radiation, in the sense that the tracks of the electrons ejected from the atom which emits the quantum of radiation and that of the electron ejected from the atom which absorbs the radiation may under suitable conditions be identified.' When supplemented twenty years later by the photographic emulsion technique and ten years later again by the bubble chamber method, this possibility of observing and studying individual quanta of radiation as well as individual particles of matter, whether charged or uncharged, has opened up whole new fields of fundamental physics.

In the second paper Wilson reproduces 17 beautiful photographs of β-rays from radioactive sources, together with some more secondary β-rays produced by X-rays. He finds that the tracks of fast β-particles are very nearly straight over distances of several centimetres. 'In the last 4 cm of the range, deviations are large and of three kinds: (*a*) sudden deviation often through large angles up to 180°, the result of a close approach to the nucleus of an atom: (*b*) the sudden deviation up to 45°, due to a close approach to an electron which is in consequence ejected to form a branch track, generally approximately at right angles to the primary track; (*c*) gradual deviation due to an accumulation of small deviations of (*a*) or (*b*) type, individually too small to be detected.' The variation of range with energy, as reported in the previous paper, was shown to be in agreement with Whiddington's law ($R \propto v^4$) and the primary ionization was found to agree with Thomson's theoretical expression, as did also the number of branch tracks. The number of deflexions exceeding 90° agreed with Rutherford's scattering formula.

The rather regular curvature of some slow β-ray tracks was commented upon and held to be inexplicable by a succession of randomly-oriented deflexions, and so suggested some kind of bias. Both Compton and Shimizu had noticed these curved tracks in Wilson's photographs and had suggested that the bias might be due to the electron being set in rotation. Wilson preferred to think that the effect might be due to the secondary radiation produced by the β-particles continually overtaking it and affecting the nature of the subsequent encounters of the β-particles. 'The bias causing the curvature may then not lie in the β-particle, but in the field of radiation in which it is moving.' As far as the writer known, this phenomenon of the appearance of a rather constant curvature of some β-ray tracks has never been re-investigated; there is no actual quantitative proof available that it is a purely random phenomenon, though this is far the most likely explanation: possibly the few smoothly curved tracks are merely those which have been visually selected out of a large number showing rather irregular curvature, and represent therefore a rare succession of random deflexions. It is, of course, true that a special kind of bias in multiple scattering of fast electrons is now well known, due to the interaction of the electron spin with the coulomb field of the scattering atom. However, existing theory would by no means predict the large effects noticed by Wilson.

In the same year, 1923, Wilson published a short paper entitled 'On some α-ray tracks' in the *Proceedings of the Cambridge Philosophical Society* (**21,** 405, 1923). Thorium emanation was used in the chamber and photographs were obtained showing the tracks of the α-rays from both the thorium emanation and the thorium A atom (lifetime $0 \cdot 14$ s). In certain cases the track of the first α-particle is complete, but the track of the second is split into positive and negative parts at its start due to the first track robbing the locality of water vapour. From the separation of the two parts of the track, the life of the particular thorium A atom could be measured. Observation and calculation was made of the δ-rays accompanying fast α-ray tracks.

This was Wilson's last paper giving results of his own observation of atomic phenomena with the cloud chamber. However, in 1933 he described a new and very simple type of cloud chamber, in which the moving piston was replaced by a fixed wire gauze behind which a rubber diaphragm was used to make the expansion. Wilson wrote that this type of chamber was very easily constructed and was much less restricted as to size and shape than the conventional cloud chamber. In fact very many of the cloud chambers, small and large (some with a volume of a cubic metre), which have been built since then have been derived essentially from this simple idea—and how obvious when once thought of.

Two years later, with J. G. Wilson (now Professor of Physics at the University of Leeds), C. T. R. Wilson described two more types of chamber. The first was a flat chamber designed to fit between the poles of a magnet, and at the moment of the expansion to fall freely under gravity so as to get clear of the pole pieces to allow a photograph to be taken. It was verified that the distortion of the tracks by convection was small because the convection forces vanish during a free fall. The actual expansion was made radially by means of a cylindrical slit system made of slate rings with gaps between them.

Though neither the falling nor the radial chamber has found any wide application, this conception was typical of Wilson's extreme originality concerning simple and ingenious ways of doing the things he wanted to do and of his clear understanding of the fundamental physical basis of the functioning of his apparatus.

C.T.R. was a lone worker at heart and had no research students working with him until the middle 1920s. C. F. Powell (now Professor of Physics at the University of Bristol) worked under him from 1925 to 1928 on the problem of the temperature variation of the supersaturation required to produce different types of condensation in air saturated with water vapour. Surprisingly this work had an important technological result, as it led to an explanation of a discrepancy between the calculated and observed flow of steam through nozzles and so became a standard feature of the theory of the steam turbine.

T. W. Wormell started work in 1925 with C.T.R. on atmospheric electricity problems. He wrote a number of important papers concerning lightning and its relation to the earth's electric field, and in all of which Wormell expresses his gratitude for C.T.R.'s help and guidance.

During and after World War I Wilson applied his unique understanding of electrostatic phenomena to problems of the protection of airships and balloons from the danger of fire due to electric discharges and lightning flashes. Several confidential papers were published by the Advisory Committee on Aeronautics.

P. I. Dee (now Professor of Physics at Glasgow) on graduating in 1926 worked with C.T.R. on cloud chamber problems, including the accurate counting of ions. This led Dee in 1932, when the neutron was discovered, to look for a direct interaction between neutrons and electrons, which was not then known not to exist.

As has already been mentioned, J. G. Wilson started work in 1933 with C.T.R. on developing the radial and freely-falling cloud chambers.

About the same time J. P. Gott worked with Wilson on various details of thunderstorm theory and carried out experiments on the capture of ions by drops falling in an electric field, and W. A. Macky investigated the distortion and rupture of drops in intense fields. Related to this was a joint paper by C. T. R. Wilson and G. I. Taylor on the bursting of soap bubbles in an electric field.

I first met C. T. R. Wilson just after World War I when I attended his lectures on light. His voice was not easy to follow and his writing on the blackboard was difficult to read, but somehow I took adequate notes and these are almost the only lecture notes of my student days to which I have repeatedly returned. He had a penetrating but very simple approach to wave phenomena, in particular to interference and diffraction effects, which he treated by the elegant use of amplitude phase diagrams. By these methods he conveyed a deep physical understanding of the Abbé theory of image formation and the closely related general relationship between the Fourier expansion of the light from a periodic object and the intensity of the resulting spectra. It may be that W. L. Bragg (now Sir Lawrence Bragg), who attended C.T.R.'s lectures just before the war, was aided by them in his later brilliant development with his father, Sir William Bragg, of the application of Fourier analysis to the elucidation by X-rays of complex crystal structures.

C.T.R.'s influence on the teaching of experimental physics in Cambridge was also profound: for many years he was in charge of the Third Year experimental class in the Cavendish Laboratory and early introduced the method of giving the students a few simple but searching experimental tasks, which demanded considerable experimental skill and had the character of minor research problems each requiring many weeks of work. This method was then in considerable contrast to the more usual practice of making the students, even in their final year, perform numerous rather stereotyped experiments. For very many years he gave devoted attention to the students who passed through the senior laboratory of the Cavendish and inspired them with a deep and lasting affection.

When I graduated in 1921, Sir Ernest Rutherford, as he then was, set me the task of using Wilson's cloud-chamber method to photograph the disintegration of atomic nuclei by α-particles. This I succeeded in doing in 1924, and for the next fifteen years I was personally engaged in researches with the cloud chamber. So of all my generation I perhaps am the most deeply indebted to C.T.R.'s genius which enabled him to perfect the first method of revealing the tracks of individual particles. Amongst the many discoveries made by the use of Wilson's cloud chamber, outstanding were those of the positive electron, pair production and cosmic ray shower phenomena, the μ-meson and its spontaneous decay, the charged and neutral V-particles and the negative cascade hyperon.

Shortly after Wilson's retirement in 1934 from the Jacksonian Chair at Cambridge, he returned to Scotland to settle eventually at the village of Carlops near Edinburgh, within a few miles of his birthplace. He continued to work actively at the theory of thunderstorms and published his long paper in the *Proceedings of the Royal Society* at the age of 87. He was as active physically as he was mentally. At the age of 82 he climbed on Caisteall Abhail in the Mountains of Arran, and when he reached the ridge remarked that it was 64 years since he first sat there.

In 1955 at the age of 86 he learned that students of meteorology at Edinburgh were allowed to fly in aeroplanes of the Royal Air Force, and enquired: 'Would I count as a student of meteorology?' Dr James Paton, who accompanied him on all his flights, wrote of them in the following words: 'With the pockets of his windcheater stuffed with maps and sandwiches, our new student would come in by bus to the Airport in the morning for flights to the Western Isles. During the flight he would move about eagerly from one side of the Anson to the other, identifying the peaks and lochs and watching the clouds. Once we had a very rough passage through a thunderstorm and were alarmed at the effect it might have on our 88-year-old companion. We need not have worried! There he was, straining at his safety belt and peering intently at the lightning flickering around us, quite oblivious of the bumps and rolls. He revelled in this, his first opportunity of seeing—and feeling—a thunderstorm at close quarters, after having spent a life-time studying it from the ground. The keen enjoyment and interest he showed on these jaunts never ceased to intrigue the pilots and navigators. He was a perpetual source of wonder to them, ever since on his first flight he had remarked to the pilot: " Do you know that you've taken me off the ground for the first time in 86 years?" '

C.T.R.'s life was a long and happy one and he died quietly at his cottage on 15 November 1959 after a brief illness, the only serious illness in his life. Of the great scientists of this age, he was perhaps the most gentle and serene, and the most indifferent to prestige and honour; his absorption in his work arose from his intense love of the natural world and from his delight in its beauties.

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